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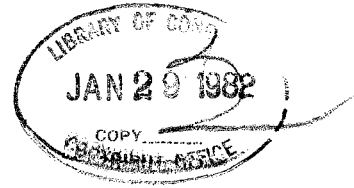
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*The American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$50.00 annually; \$20,000–\$29,999, \$42.00; \$15,000–\$19,999, \$35.00; \$10,000–\$14,999, \$25.00; below \$10,000 and joint memberships \$15.00; associate (non-historian) \$25.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 86: Class I, *American Historical Review* only, United States, Canada, and Mexico \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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## The Democratization of Russia's Railroads in 1917

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WILLIAM G. ROSENBERG

IN EXPLAINING THE FAILURE OF DEMOCRACY in Russia during the 1917 revolution, Western historians have long emphasized the weakness of representative institutions and traditions and have stressed such influences as the enormous social and economic dislocation brought on by the war, Bolshevik disdain for legality, and Russia's general cultural backwardness, particularly in the countryside. Recent research, some of it contributed by Soviet historians, has allowed these matters to be treated with considerable sophistication.<sup>1</sup> New attention has also focused on aspects of popular (mass) mobilization and the ways in which "bureaucratization from below," especially through networks of popular councils like the soviets, accentuated social polarization, undermined government legitimacy, and helped create a civil war mentality that Lenin's supporters used to great advantage.<sup>2</sup> Still, the prevailing explanation for the failure of democracy remains closely tied to the views of well-known contemporaries like Vasili Maklakov, the Provisional Government's ambassador to France, or Alexander Kerensky, its last prime minister, who were convinced that Russia received prematurely "more freedom than it could manage" and that "only conspiracy and treacherous armed struggle broke up the provisional regime and stopped the establishment of a democratic system."<sup>3</sup>

An earlier version of this essay was presented at the Second World Congress for Soviet and East European Studies, held in Garmisch-Partenkirchen, West Germany, September 1980. I am especially grateful to David Bien, Heather Hogan, Ronald G. Suny, and Allan Wildman for their constructive criticisms and thoughtful comments. Much of the research was done with support from the Center for Russian and East European Studies, University of Michigan, and the Joint Committee on Soviet Studies of the American Council of Learned Societies.

<sup>1</sup> See especially Marc Ferro, *La Révolution de 1917: La Chute du tsarisme et les origines d'octobre* (Paris, 1967); George Katkov, *Russia 1917: The February Revolution* (New York, 1967); Roger Pethybridge, *The Spread of the Russian Revolution: Essays on 1917* (London, 1972); Alexander Rabinowitch, *The Bolsheviks Come to Power* (New York, 1976); Mikhail Reiman, *Russkaia revoliutsiia, 23 fevralia–25 oktiabria 1917*, 2 vols. (Prague, 1968); and Allan Wildman, *The End of the Russian Imperial Army* (Princeton, 1980). For the best Soviet work on the revolution, see E. N. Burdzhakov, *Vtoraia russkaia revoliutsiia: Vosstanie v Petrograde* (Moscow, 1967), and *Vtoraia russkaia revoliutsiia: Moskva, front, periferiia* (Moscow, 1971). Important contributions have also been made by L. S. Gaponenko, A. Ia. Grunt, V. Ia. Lavrychev, P. N. Pershin, and P. V. Volobuev, among others.

<sup>2</sup> Oskar Anweiler, *Die Rätebewegung in Russland, 1905–1921* (Leiden, 1958); John Keep, *The Russian Revolution: A Study in Mass Mobilization* (New York, 1976); Marc Ferro, "The Birth of the Soviet Bureaucratic System," in R. C. Elwood, ed., *Reconsiderations on the Russian Revolution* (Cambridge, Mass., 1976), 100–32.

<sup>3</sup> Maklakov, *Vlast' i obshchestvennost' na zakate staroi Rossii* (Paris, 1938), 451–53; and Kerensky, "The Policy of the Provisional Government of 1917," *Slavonic and East European Review*, 31 (1932): 19.

This view is both conceptually and analytically weak. To be sure, the institutional weakness of the provisional regime—by now, surely, well enough known to require no rehearsal—played a vital role in the transition to October. So did the war, Bolshevik organizational strength, Russia's economic condition, the social and cultural isolation of the peasantry, and the absence of strong democratic political traditions. But, to understand democracy's failure in 1917 (as well as to comprehend, incidentally, the major contours of the early Bolshevik state), one must recognize that democracy was not merely—perhaps not even largely—a matter of representative politics or civil liberties in the Russian revolution but concerned mass participation in social affairs, popular initiative in resolving a wide range of issues, and the assault on what might be called the social corollaries of autocratic politics: status differentials, privilege, income and welfare inequities, the power of social elites, and, above all, the arbitrariness with which this power was often exercised. As such, democracy or, more precisely, the process of democratization involved decentralized, popular supervision over all aspects of social and public administration by new, mass institutions, which were not necessarily electoral or even formally representative. In the language of the revolution itself, to be a "democrat" was to support popular authority (control by "the democracy") over the dominance of Russia's traditional elites (control by "the society").

The question of democracy's "failure," therefore, needs to be conceptualized and explored not so much in terms of elections, political legitimacy, or Bolshevik conspiracy but in terms of how "democratic" mass organizations performed, the psychology and social objectives of their members, and the ways in which these groups may or may not have been recognized in various quarters as proper components of a new revolutionary order. Most important is a range of issues touching the relationship between democratic mass institutions and the government's formal commitments to democracy in the more common political sense of the term. What, for example, did the end of political authoritarianism imply for authoritarianism in other areas of Russian life? What was the democratic way to organize the workplace in Russian industry? Was it an unwarranted extension of equality to allow employees a voice in factory administration, or did limiting their role to welfare matters simply reinforce a lack of interest and subservience that contributed to low productivity and enhanced authoritarian traditions?

The range of questions can, of course, be expanded, but I focus on labor and industry because my aim here is to explore in some detail the process of democratization as it emerged during 1917 in one vital industrial sector, the railroads, and to examine the relationship between this process and the values and institutions that the Kerensky government struggled unsuccessfully to defend. In doing so, I hope to test the hypothesis that a regime more sympathetic to democracy in the sense of mass participation, perhaps one in which Petrograd Soviet leaders, rather than liberals, were vested early on with full power, might have prevented the failure of political democracy at high levels and facilitated a different revolutionary outcome. I think that it would not; indeed, the very "success" of workers' control on the railroads weakened resistance in this crucial sector to the

Bolsheviks' coming to power. More generally, and most importantly, I hope to demonstrate that the failure of democracy in 1917 can only be properly understood in terms of broad conceptualization of Russia's democratic experience.

I CONCENTRATE ON THE RAILROADS for several reasons. First, the railroads constituted Russia's most important industry in 1917, both economically and politically. Stretching over some sixty thousand kilometers of track and employing somewhere in the neighborhood of one million people, railroads were the lifeline of Russia's army, the crucial supply link to its cities, the lynchpin in the system of distributing industrial goods and raw materials, and a nerve system of telegraphic and other communication. When not in service, all significant political and economic activity came to a grinding halt. In 1905, a massive general railroad strike amounted to just that, and the strike was one of the decisive steps in forcing the tsar to issue his October Manifesto, promising a constitutional regime. No regime could survive for long if Russia's trains were not running, as Lenin himself recognized. Even armed force could not compensate for the special skills required in railroad operations.

Russia's railroads also epitomized the process of workers' control in 1917. In no other industrial or social sector was there a more dramatic proliferation of influential mass organizations—workers' committees, line congresses, trade unions, craft organizations, and more. By early April there was hardly a depot, shop, station, or administrative office that did not have its own workers' committee. Every one of the country's major private and state-owned lines had "line executive committees" supervising the operation of central administrative offices. On some lines, the committees personally selected new administrative officers (*nachal'niki*) to replace those appointed by the company or those (on state lines) who resigned with the tsar's abdication. On virtually all others, administrative effectiveness depended on the committees' sanction. As many as twenty-five different railroad lines throughout the months between February and October even held representative workers' congresses that established railroad policy, organized workers into additional "democratic" committees, and issued specific directives on railroad operations.<sup>4</sup> Examining the railroads thus permits the exploration of a full range of "democratic" activity "from below."

Third, all Russian railroads in 1917, state-owned and private, were legally under the full control of the government, specifically the Ministry of Transport.<sup>5</sup> And, unlike the situation in other branches, transport officials even in Prince Lvov's first cabinet were committed to extensive employee participation in industry affairs and were strong partisans of a regime closely allied with soviet leadership. From March through July, the transport post was held by the left Kadet, Nicholas Nekrasov, a bitter enemy of conservative liberals even within

<sup>4</sup> *Vikzhel' v oktiabr'skie dni* (Petrograd, 1918), 86. *Vikzhel'* was an acronym for *Vserossiiskii ispolnitel'nyi komitet zheleznodorozhnikov* ("All-Russian Executive Committee of Railroad Workers").

<sup>5</sup> Approximately 70 percent of Russian trackage in 1917 was state-owned. For general analyses of the condition of Russian railroads in this period, see I. D. Mikhailov, *Evolutsiia russkogo transporta, 1913–1925* (Moscow, 1925), 64–75; and N. Vasil'ev, *Transport Rossii v voine, 1914–1918 gg.* (Moscow, 1939).

his own party and a champion of workers' control.<sup>6</sup> Working closely with Kerensky and especially with George Plekhanov, the venerable elder of Russian Marxism whom he appointed to head his most important ministerial commission, Nekrasov strove to create precisely the responsive administration envisioned by those in favor of a popular, worker-oriented regime. Studying the railroads thus permits rather careful testing of the "alternative model" hypothesis.

Finally, railroad workers as a group were not politically radical in 1917, despite the vital role they had played in precipitating the constitutional reforms of 1905. On the contrary, many leftists thought they displayed overwhelmingly a "petty bourgeois" mentality. Bolsheviks had substantial influence in major repair shops, but even here railroaders seemed far less militant than their comrades in other metal-working industries. The vast number of clerks, trainmen, station officials, baggage handlers, engine personnel, and the like appeared solidly behind the Socialist Revolutionaries (SRs), whose supporters also dominated the all-Russian union's executive committee (*Vikzhel*). Railroaders were, in short, "loyalists" in 1917. Kerensky and others expected them to fight tooth and nail against any unilateral Bolshevik seizure of power. Many have believed this did not happen because Lenin immediately entered into negotiations with *Vikzhel* leaders and for a time seemed ready to accept their demand for a coalition regime.<sup>7</sup> But the very range of antagonisms and problems associated with railroad democratization more than likely made a general strike on the lines highly improbable in October, even had one been called. Thus, exploring this particular aspect of the Russian revolution closely should facilitate a clearer understanding of the full dimensions of Russia's democratic experience as a whole.

RUSSIA'S DEMOCRATIC EXPERIENCE began in the February Revolution with extremely widespread support for a change of government and the creation of some form of representative rule. The strikes and demonstrations in the streets of Petrograd and the crucial decision of garrison soldiers not to suppress the disorders were popular reflections of the deep disaffection running through virtually all levels of Russia's highly stratified social order. Even many monarchists, shocked by the tsar's abdication, recognized the need for a government "commanding everyone's confidence and able to exalt Mother Russia."<sup>8</sup>

This general enthusiasm for change tended to obscure the fact that the nature

<sup>6</sup> Nicholas Vissarionovich Nekrasov had been mayor of Tomsk and a professor at the Tomsk Technological Institute before being elected to the Third and Fourth State Dumas. He was a member of the Kadet Central Committee and, despite his resignation in May, was re-elected at the Eighth Party Congress. From July until the end of August, he served as minister of finance and deputy minister-president, and, for a brief time in September and October, he was governor general of Finland. He remained in Russia after the October Revolution and served throughout the 1920s in various minor administrative posts. On the Kadet party in general in 1917, see William G. Rosenberg, *Liberals in the Russian Revolution* (Princeton, 1974).

<sup>7</sup> For a detailed discussion of these negotiations, see *Vikzhel' v oktiabr'skie dni, passim*; and, for less extended treatments, see Pethybridge, *The Spread of the Russian Revolution*, 50-52; and Edward H. Carr, *The Bolshevik Revolution, 1917-1923*, 2 (New York, 1952): 394-97. Also see W. R. Augustine, "Russia's Railwaymen, July-October 1917," *Slavic Review*, 24 (1965): 666-79.

<sup>8</sup> M. V. Rodzianko (president of the Fourth Duma and chairman of the Temporary Duma Committee), Speech to the Preobrazhenskii regiment, as reported in *Izvestiia Revoliutsionnoi Nedeli*, February 28, 1917.



of the disaffection characterizing Russian society was actually of quite different sorts. Among industrial leaders, Duma representatives, leading gentry elements, and even many state and army figures—that is, “privileged Russia” in general—concern focused on Russia’s ability to prosecute the war. Victory was vital to Russia’s national interest. The February Revolution’s importance to Russia’s elite, then, centered on the expectation that the political and administrative impediments to military success could now be overcome. This required both able people in responsible positions and the rationalization of Russia’s political and economic apparatus, long the goals of such groups as the War Industries Committees and the Union of Towns and Zemstvos.

Quite a different range of disaffection, however, characterized “ordinary” Russians in early 1917. The impact of the war on the cost of living, on the availability of foodstuffs, and on the cohesion of family life was compounded initially by the indiscriminate mobilization of skilled workers and peasant heads of household and later by the autocracy’s inability to manage effectively Russia’s transport system, the distribution of scarce materials, or even the proper supply of troops. All such problems produced real privation as well as a growing sense of insecurity. Even temporary factory closings and lockouts threatened workers’ well-being (particularly that of the older men and the women), just as orders to the front threatened and frightened garrison soldiers.

Popular disaffection also stemmed from the virtually unlimited powers that many foremen, factory administrators, and other officials held over their employees, paralleling in many ways the power of army officers over their troops. Authoritarianism in the workplace was as little bound by judicial or other restraints as that in the army or government. On the railroads, closely regulated because of their strategic and economic importance, an especially broad range of arbitrary sanctions was used to keep order. On one line, employees were “strictly forbidden to bring charges of any sort in a civil court,” since the “resolution of all grievances” was the prerogative of line officials; on another, employees were “strictly forbidden to go into debt,” since indebtedness was “immoral” and indicated that “an employee cannot live within his means.”<sup>9</sup> Violations brought immediate dismissals. There were also fines, detention, and even imprisonment, all imposed by petty autocrats whose decisions could not be appealed. During one week in January 1917, for example, twenty-six workers on the Riazan-Urals railroad were jailed for an entire week for lateness; forty-eight others were held from one to fourteen days for “negligence on duty and failure to follow regulations.”<sup>10</sup> The threat of such sanctions also pressed many railroaders to take dangerous chances—switching and coupling cars on the run, shortcutting road repair work in order to finish it on schedule, running engines without proper repairs. Railroad work, particularly in the yards and shops, was one of Russia’s bloodiest occupations, a fact many workers tied directly to the arbitrary powers of their *nachal’niki* (“bosses”), whom they derisively referred to as *tsar i bog*—“caesar and god.”

<sup>9</sup> *Zheleznodorozhnik* (Petrograd), 211/30 (August 1, 1917).

<sup>10</sup> *Vestnik Riazansko-Ural’skoi Zheleznoi Dorogi*, 3 (January 20, 1917).

Consequently, among ordinary Russians, especially in urban centers like Moscow and Petrograd, revolution at first had little to do with the question of military victory and only partly concerned the establishment of democratic political institutions in the Western sense. Rather, concern here focused overwhelmingly on ending the power of arbitrary authorities and on taking control, in one form or another, over whatever seemed directly to affect popular welfare. If institutions like the Petrograd Soviet set themselves such supervisory tasks in terms of Provisional Government policies, creating the familiar pattern of "dual power" at the national level, equally consequential was the simultaneous and spontaneous creation of literally thousands of local workers' and soldiers' committees to secure new social relations, material benefits, and individual security.<sup>11</sup>

Moreover, while government-soviet relations became immediately problematic, particularly on the question of war aims, the ways in which these mass organizations reflected differences in the meaning of democracy and revolution were at first barely perceptible. Indeed, workers' committees were initially welcomed by Prince Lvov's cabinet as the appropriate organs for resolving labor-management conflicts and bringing new harmony to Russian industry.<sup>12</sup> This was especially the case with the railroads. The Kadet transport minister, Nicholas Nekrasov, a well-known Central Committee member and spokesman for the party's left wing, had long been an advocate of workers' rights. He believed not only that the revolution "dictated a basic transformation of all institutions" but that this transformation also had to involve popular participation at all levels. Nekrasov saw the roots of railroad deficiencies in excessive centralization, bureaucratic isolation, and *proizvol*—the arbitrary and capricious behavior of railroad officials. "Democratization" was thus the "first task of the new regime," while "democratizing the entire railroad structure," by which Nekrasov meant involving workers directly in line operations, was the "primary means for renewing Russian transport."<sup>13</sup> The result of both of these processes, Nekrasov hoped, would be cooperation, mutual respect, and "a government enjoying the confidence of the people who have called us to power" as well as an improvement in railroad operations.<sup>14</sup> In his view, and in that of Soviet leaders like Nicholas Chkheidze, Michael Skobelev, George Plekhanov, and others, the danger of the

<sup>11</sup> See Paul H. Avrich, "Russian Factory Committees in 1917," *Jahrbücher für Geschichte Osteuropas*, 2 (1963): 161–82; D. A. Tseitlin, "Fabrichno-zavodskie komitety Petrograda v fevrale-oktiabre 1917 goda," *Voprosy Istorii*, 11 (1956): 86–96; A. M. Pankratova, *Fabzavkomy Rossii v bor'be za sotsialisticheskuiu fabriku* (Moscow, 1923); and the important collection of materials *Rabochii kontrol' i natsionalizatsiia promyshlennykh predpriatii Petrograda v 1917–1919 gg.*, 1 (Leningrad, 1947). For a general discussion of the literature, see William G. Rosenberg, "Workers and Workers' Control in the Russian Revolution," *History Workshop Journal*, 5 (1978): 89–97.

<sup>12</sup> See, for example, the optimistic speech by A. I. Konovalov, minister of trade and industry, in *Vestnik Vremennago Pravitel'stva*, March 7, 1917; cabinet discussions reported in *Zhurnaly Zasedanii Vremennago Pravitel'stva*, 14 (March 10, 1917); and the general reaction to the agreement on working conditions reached between a Petrograd Soviet committee and the Petrograd Society of Manufacturers on March 10 in, especially, *Rech'*, March 11, 1917.

<sup>13</sup> *Rech'*, March 10, 1917; *Vestnik Putei Soobshcheniia*, 11 (March 18, 1917); and *Rech'*, March 29, 1917. Nekrasov communicated his views directly to the railroad workers by teletype; see, for example, Telegram 1788, *Vestnik Ekaterininskoi Zheleznoi Dorogi*, 489/90 (March 4–12, 1917).

<sup>14</sup> *Vestnik Putei Soobshcheniia*, 11 (March 18, 1917): 49–50.

new government becoming isolated from popular opinion was as threatening to the success of political democracy as the danger of anarchy or political extremism.

Support for administrative decentralization and employee participation in railroad affairs was shared by the widely respected Conference of Railroad Engineers, which argued in its journal for a shift of responsibility for day-to-day operations to local branch and section organizations, and by the All-Russian Trade Industrial Congress, which met in Moscow on March 19 and passed a resolution endorsing representative "supervisory organs" on the railroads. Responsible local committees of informed, competent employees were seen as the basic answer to "excessive and unworkable central control."<sup>15</sup> General S. A. Ronzhin, a leading railroad specialist, thought reorganizing railroad operations was "absolutely and unconditionally needed"; even a meeting of railroad supervisors in Moscow on March 5 endorsed the idea of local committees to help unsmarl transport problems.<sup>16</sup>

Such a view was certainly compatible with the outlook of Russia's thousands of railroad workers themselves, who in the first weeks of March seized the revolutionary moment to organize literally hundreds of workers' committees in virtually every major junction, depot, or administrative center. As early as March 3, workers on the showcase Nikolaev line (between Moscow and Petrograd) organized a "Temporary Line Committee" and issued telegrams calling for worker organizations throughout the system.<sup>17</sup> Their example was followed elsewhere almost at once, as news traveled to every corner of the country on the railroad telegraph service. In Ekaterinoslav on March 4, employees of the vital Ekaterininskaia railroad, serving the Don coal basin, issued their own "Order Number One" to announce the formation of a "Provisional Line Committee" with responsibility for controlling railroad operations and convening a workers' congress to organize permanent mass organizations.<sup>18</sup> In Kharkov, Southern railroad workers held their first "Provisional Line Committee" meeting on March 6, and, on the same day in Riazan, workers of the Riazan-Urals railroad committee dispatched "agitators" up and down the line to help organize similar groups and secure support for the government.<sup>19</sup> By the end of March, according to one observer, the drive for organization "so overcame the masses of railroad workers throughout Russia that there did not seem to be a single line, district, or service that remained unorganized."<sup>20</sup>

That this remarkable, spontaneous "democratization" of the lines, as it was heralded at the time, did not initially appear to be at variance with the govern-

<sup>15</sup> *Izvestiia Sobraniia Inzhenerov Putei Soobshcheniia*, 1 (1917): 11; and *Proizvoditel'nyiia Sily Rossii*, 9-10 (1917): 36-37.

<sup>16</sup> Ronzhin, "Zheleznyia dorogi v voennoe vremia" (Palich, Yugoslavia, 1915), 15, MS in the Hoover Institution, Stanford, Calif.; and V. G. Izgachev, "Revoliutsionnoe dvizhenie zheleznodorozhnikov Zabaikal'ia v 1917 godu," *Chitinskii Gosudarstvennyi Pedagogicheskii Institut: Uchenye Zapiski*, 3 (1958): 27.

<sup>17</sup> *Izvestiia* (Petrograd), March 4, 1917.

<sup>18</sup> *Vestnik Ekaterininskoi Zheleznoi Dorogi* (Ekaterinoslav), 489/90 (March 4-12, 1917).

<sup>19</sup> *Vestnik Iuzhnykh Zheleznykh Dorog* (Kharkov), 13 (March 27, 1917); and *Vestnik Riazansko-Ural'skoi Zheleznoi Dorogi* (Saratov), 13-14 (April 12, 1917).

<sup>20</sup> A. Taniaev, *Ocherki dvizheniia zheleznodorozhnikov v revoliutsii 1917 g.* (Moscow, 1925), 4.

ment's own objectives is a fact of considerable importance in understanding later conflict. Railroad committees seemed to be staffed on the whole with responsible employees committed to "maximum labor productivity," "complete dedication to work," and "strict labor discipline" (Omsk line). They insisted on law and order, and many organized workers' militias to protect the lines in place of the tsarist militia (Moscow-Vindavo-Rybinsk, Riazan-Urals, Moscow-Kursk, Southern, and other lines). A few worked with central line administrations in removing immediate traffic bottlenecks (Northern, Chinese Eastern, and Omsk lines). Committees often appointed persons to replace tsarist administrators who had fled their posts, sometimes in conjunction with management (usually private lines), sometimes not (Moscow-Kazan and other state lines).<sup>21</sup> Most important to those in high places, the railroads seemed institutionally and ideologically committed to close soviet-government relations, the solution Nekrasov and others sought to the problem of dual power. "We consider the Soviet the true representative of the revolutionary proletariat," delegates to the first "Line Congress" of the Moscow-Vindavo-Rybinsk railroad resolved in late March, for example, "but we pledge full support and solidarity to the Provisional Government as well while it fulfills its obligations to the country and works closely with the Soviet."<sup>22</sup> Similar declarations came from other congresses of railroad delegates, which began to convene line by line on a number of roads in late March and April to organize "permanent" central line committees, confident of government support.<sup>23</sup>

This support was, to be sure, somewhat circumscribed. Nekrasov telegraphed railroaders that "the degree and manner of employee representation in railroad administration" could not be resolved officially until the nation elected a permanent government and asked that "no steps be taken immediately that might interfere with regular railroad operations."<sup>24</sup> Others, like the engineers, stressed workers' participation but not supervisory control. The significant point is not, however, that support "from above" was qualified but that railroaders presumed broad backing for their efforts and saw in this an apparent unity between government and soviet leaders. As Ekaterininskaia railroad workers wired enthusiastically back to Nekrasov, "not only" would they "refrain from independent steps," but their own newly elected soviet of line deputies would also "pass a special resolution to this effect!"<sup>25</sup> And, as the *Izvestiia* of railroad workers in Moscow reported, "even our supervisors on private lines as well as those under state control" recognized the necessity for organization, "and so we have or-

<sup>21</sup> *Professional'noe dvizhenie na Moskovsko-Kazanskoi zheleznoi doroge, 1917-1927* (Moscow, 1928), 8-9; *Vestnik Omskoi Zheleznoi Dorogi*, 12 (March 25, 1917); *Vestnik Iuzhnykh Zheleznykh Dorog*, 13 (March 27, 1917); *Golos Zheleznodorozhnika*, June 7, 1917; *Izvestiia Ispol. Kom. Moskovsko-Kazanskoi Zheleznoi Dorogi*, 3 (June 13, 1917); and *Vestnik Riazansko-Ural'skoi Zheleznoi Dorogi*, 23 (June 20, 1917).

<sup>22</sup> *Golos Zheleznodorozhnika*, June 4, 1917.

<sup>23</sup> The delegate congress of the Northwestern Railroad resolved on April 6, for example, to "recognize the Soviet of Workers' and Soldiers' Deputies as the sole representative of the interests of railroad workers" and at the same time pledged full support to the Provisional Government "while it enjoys the trust of the Petrograd Soviet"; see *Zemlia i Volia* (Moscow), April 12, 1917.

<sup>24</sup> *Vestnik Ekaterininskoi Zheleznoi Dorogi*, 489/90 (March 4-12, 1917).

<sup>25</sup> *Ibid.*



ganized."<sup>26</sup> On the Southern line, workers even declared strikes to be an "unacceptable" method of resolving grievances "before the successful completion of the war."<sup>27</sup>

There was also some indication that employee participation was helping to ease the transport crisis. The number of freight cars hauled in daily service increased from an average of sixty-six thousand in February to seventy-three thousand in March, seventy-five thousand in April, and seventy-six thousand in May. Statistics on car loadings showed increases of 14 and 19 percent for March and April in comparison to the figures for February. The number of locomotives in service also increased.<sup>28</sup> What seemed most impressive about this process, moreover, was not merely the level of cooperation between government and labor but also the manner in which personnel at all levels apparently functioned well under the supervision of representative workers' committees, despite the incredible number of these organizations that had sprung up on the lines. "Democratization" in the sense of broad worker participation in railroad industry affairs thus seemed capable of achieving both what the upper reaches of society understood as the goals of a democratic Russia (the successful prosecution of the war, responsible government, and the rule of law) and what most workers on the railroads and elsewhere saw as a democratic solution to the root causes of their own disaffection (material deprivation, personal insecurity, and the extensive and arbitrary powers of officials).

APPEARANCES CAN BE—AND WERE—MISLEADING. Beneath surface unity, powerful countervailing pressures were already moving to disjoin this "loyalist" labor army from supporting government and soviet leaders alike. At the outset, these pressures had little to do directly with national politics. The first major crisis of the revolutionary period—in April, over dual power and the war—led to a redefinition of Russia's war aims and the formation of a liberal-socialist coalition. But these changes, if anything, seemed to strengthen the position of men like Nekrasov and their hopes for a responsive, popular regime. What was happening instead was twofold:

First, the proliferation of workers' control organizations was creating what might be called a broadly based "syndicalist mentality" on the part of committee members, who were reluctant to yield to any outside authority and increasingly willing to countenance popular actions without regard to normative judicial or governmental processes. With Lenin's return to Russia in early April, the Bolsheviks played an increasingly important role in stimulating these attitudes, but such views were not of their making. At the first Conference of Petrograd Factory Committees in May, for example, which brought together more

<sup>26</sup> *Izvestiia Ispolnitel'nago Komiteta Moskovskago Uzla, Moskovsko-Kazanskoi Zheleznoi Dorogi* (Moscow), 2 (June 6, 1917).

<sup>27</sup> *Vestnik Iuzhnykh Zheleznykh Dorog*, 13 (March 27, 1917). Also see *Russkiiia Vedomosti*, May 30, 1917, June 6, 1917; and *Vestnik Riazansko-Ural'skoi Zheleznoi Dorogi*, 23 (June 20, 1917).

<sup>28</sup> I. D. Mikhailov, *Transport: Ego sovremennoe sostoianie* (Moscow, 1919), 32–33; and *Ekonomicheskoe polozhenie Rossii nakamune velikoi oktiabr'skoi sotsialisticheskoi revoliutsii*, 2 (Moscow, 1957): 226–28.

than 560 delegates from dozens of plants, Jacob Sverdlov, Grigorii Zinoviev, and Lenin himself led a blistering attack on the Menshevik minister of labor Skobelev and others, like Nekrasov, who insisted on a peaceful resolution of industrial conflict. Not surprisingly, Bolshevik railroaders echoed these sentiments on a number of lines, particularly those like the Nikolaev and the Moscow-Kursk, which served the capitals. Yet the sense of autonomy and disdain for outside authority that was coming to characterize the committees' outlook stemmed more from the very power and importance these organizations were acquiring in daily affairs. It made little sense to waste time appealing to government or even soviet agencies when crucial issues could and had to be decided on the spot. And particularly on the railroads, where workers' control had official sanction and seemingly broad government-soviet support, a syndicalist approach appeared to be precisely suited to Russia's revolutionary needs.

Second, these attitudes on the part of the committees intensified a growing friction between different categories of railroad workers themselves, a friction not readily apparent to those who tended to see all railroaders as a unified and harmonious labor "family," as it was often called. In fact, this family was highly stratified by profession, income, well-being, and a variety of social distinctions associated with rank. These divisions had a structural basis in the ten or so individual "services" into which all railroad workers were categorized. The two largest of these, the traction service (*sluzhba tiagi*) and the roadway service (*sluzhba puti*) included the largest numbers of shop workers and ordinary laborers; the operations, telegraph, supply, administration, and other services had relatively small percentages of the total railroad work force but had the majority of educated clerks and white-collar workers, "grey-collar" train personnel (conductors, trainmen), station masters, ticket sellers, and the like, many of whom prided themselves (with little obvious reason) on the importance and social distinction of their jobs, as "evidenced" by their fancy uniforms. Engine drivers in particular carried with them the aura of a labor elite—an "aristocracy" in the use of the term as popularized by the Bolsheviks. Comprising less than 4 percent of the total railroad work force, engine drivers earned on the average more than twice the wages of firemen, who worked with them in the cabs in a clearly subservient relationship, and almost five times the wages of greasemen, car cleaners, or ordinary watchmen. Along with shop and depot workers and ordinary laborers of the roadway service, personnel in these categories stood at the bottom of the railroaders' social spectrum and were often treated contemptuously by the "aristocratic" drivers and the officious petty "service intelligentsia" in ticket bureaus and administrative offices up and down the lines.<sup>29</sup>

The organization of workers' committees on the railroads reflected, and soon

<sup>29</sup> A. G. Rashin, *Chislennost' i sostav rabotnikov zheleznodorozhnogo transporta k kontsu 1920 g.* (Moscow, 1921), 5–11; and V. Rachinskii, *Zheleznodorozhnyi transport v 1913 g.* (Moscow, 1925), 146–50. Also see P. F. Metel'kov, *Zheleznodorozhniki v revoliutsii* (Leningrad, 1970), 23–24. The role of engine drivers in the railroad work force elsewhere is discussed in two interesting essays: F. Caron, "Essai d'analyse historique d'une psychologie du travail: Les Mécaniciens et chauffeurs de locomotive du Nord de 1850 à 1910," *Le Mouvement Social*, 50 (1965): 3–40; and Margot B. Stein, "The Meaning of Skill: The Case of the French Engine Drivers, 1837–1917," *Politics and Society*, 3–4 (1978): 399–428.

reinforced, these occupational and social divisions. The evidence indicates that most committees sprang up initially either among yard and repair workers in shops and depots or among clerks and other employees in administrative centers; but administrative personnel soon began to dominate the provisional and then the permanent central line committees—that is, the *principal* organs of workers' control.<sup>30</sup> This dominance followed logically from the committees' administrative tasks, most of which required special skills and technical expertise not generally held by lower-level employees. Typically, the committees met three or four times a week to consider complex administrative issues: methods of improving traffic and productivity; the question of train lengths and their relationship to goods traffic and safety; technical deficiencies in line operations; and so forth. Like the soviets, they received a constant stream of delegations from various lower-level shop, depot, trade, and station organizations, which presented specific grievances and demands; and they generally responded either by forming special commissions or by taking various issues up directly with managerial personnel.

Yet, as rank-and-file workers organized themselves by shop and depot and as engine drivers and conductors held meetings to discuss specific professional problems (more often than not, incidentally, at the urging of the provisional central line committees and their "organizational-agitational" commissions), the question of the central line committees' own ability to defend particular workers' interests naturally came into question. This issue usually emerged first at the various congresses of line workers, which initially convened in April and early May. Here, more direct issues of pay rates for particular trades and services, hours, responsibility for line problems, personnel, and the like were raised along with political issues: support for the war and the liberty loan; elections to local dumas; and the questions of a Russian republic and the Constituent Assembly. Some delegates heatedly insisted these latter questions were not properly within the railroaders' provenance; others challenged the "territorial" (as opposed to "trades") principle on which central line committees were organized and urged their organizations to take a more partisan stance in defense of particular "professional" interests and, as a group of workers on the Moscow-Kursk line put it, to become something more than a "dead skeleton without a political soul."<sup>31</sup> On lines like the Moscow-Kursk and the Nikolaev, more radical committees responded by openly resolving to "strengthen ties with Russia's proletarians and peasants" and supporting the "equalization of the economic circumstances of all employees"; but committee members like those on the Alexandrovsk line, who early came to the realization that "antagonism between senior agents of the operations service and those on the traction service" was itself "one of the main elements" affecting line operations, clearly saw the dangers of "professionalism" at

<sup>30</sup> *Vestnik Ekaterininskoi Zheleznoi Dorogi*, 489/90 (March 4–12, 1917); *Vestnik Omskoi Zheleznoi Dorogi*, 12 (March 25, 1917); *Vestnik Iuzhnykh Zheleznykh Dorog*, 13 (March 27, 1917); *Golos Zheleznodorozhnika*, 4 (June 7, 1917); *Izvestiia Ispol. Kom. Moskovsko-Kazanskoi Zheleznoi Dorogi*, 3 (June 13, 1917); and *Zheleznodorozhnaia Zhizn' na Dal'nem Vostoke*, 26 (July 9, 1917).

<sup>31</sup> *Volia i Dumy Zheleznodorozhnika*, April 28, 1917.

the expense of a more general, territorial, and intra-trade approach to problems.<sup>32</sup>

Questions concerning the nature and limits of committee authority thus began to emerge in the spring of 1917 not only from above—in government and soviet circles—but also among railroad committee members themselves. In response to these “sectarian” pressures from below, moreover, main line organizations gradually began to press for an extension of their own power. Against management, they insisted on the right to discharge unpopular personnel (Alexandrovsk line, Chinese Eastern line, Moscow-Kursk line, and many others), although they generally accepted “suspension pending investigation” as a compromise solution. They also tried to respond to particular grievances (such as introducing a three-shift system for switching personnel and limiting the allowable running time for locomotive brigades), although here, too, there was a willingness to wait until the question was settled by the government.<sup>33</sup> More important, the committees also pressed against lower-level groups, insisting to the increasing number of professionally sectarian organizations along the lines that their own authority was, and had to be, paramount.

Consequently, new complaints soon began to be heard from below about central line committees “usurping” workers’ prerogatives, “interfering with and ignoring the competency of local groups,” or ignoring worker interests by failing, for example, to abolish piece rates in railroad shops.<sup>34</sup> In response to this growing unrest, moreover, central line committees themselves began to demand even greater unity and discipline. Some even insisted that they themselves had the right to levy fines and impose other disciplinary sanctions in the traditional way. And, while conductors, ticket sellers, engine drivers, and various other categories of workers began to sense here the reaffirmation of prerevolutionary authoritarianism on the lines (a feeling that intensified in late spring and early summer when railroad performance again began to decline), a number of central line committees, for their part, pressed the government for an even further extension of their powers. As one committee spokesman declared to government officials who believed railroad committees should restrict themselves to welfare issues, “Open your eyes! You are blind! Life has already given us the power to organize and control railroad life, and you do not even see it!”<sup>35</sup>

TO APPRECIATE HOW TROUBLESOME these charges were for Nekrasov and other supporters of workers’ control, who continued to hope that a regime responsive to popular interests could bring stability and order to revolutionary Russia, we need to recognize that democratization of the railroads still represented a “suc-

<sup>32</sup> *Zemlia i Volia*, May 3, 1917; and *Vestnik Aleksandrovskoi Zheleznoi Dorogi*, 17 (September 15, 1917).

<sup>33</sup> *Golos Zheleznodorozhnika*, June 29, 1917; *Volia i Dumy Zheleznodorozhnika*, April 28, 1917; and *Zheleznodorozhnaia Zhizn’ na Dal’nem Vostoke*, 10–11 (March 15, 1917).

<sup>34</sup> See, for example, the accounts of committee meetings in *Vestnik Aleksandrovskoi Zheleznoi Dorogi*, 17 (September 15, 1917); *Vestnik Omskoi Zheleznoi Dorogi*, 12 (March 25, 1917); *Vestnik Ekaterininskoi Zheleznoi Dorogi*, 504/28 (July 27, 1917); *Vestnik Riazansko-Ural’skoi Zheleznoi Dorogi*, 13/14 (April 12, 1917); and *Zemlia i Volia*, April 26, 1917.

<sup>35</sup> *Volia i Dumy Zheleznodorozhnika*, April 28, 1917.

cess" in the spring of 1917, in contrast to deteriorating situations elsewhere. In the countryside, peasants were refusing to cooperate with government-organized land committees and were seizing estates with increasing frequency and rapacity. The new socialist minister of labor Skobelev, whose post was established with the organization of the first coalition cabinet in early May, found it almost impossible to resolve critical industrial conflicts, particularly in the Don region and the Urals. By early summer, almost half of the metal-working and mining plants in these areas had closed.<sup>36</sup> On April 23, the provisional regime had officially recognized the right of workers to set up committees in industrial enterprises of every kind, but in many places factory owners responded with lockouts, blamed (with at least some degree of legitimacy) on the absence of materials and the increased costs of production. For those who valued the revolution essentially in terms of military victory and responsible, representative political institutions, these developments were ominous in the extreme. For those oriented as well toward popular interests, the growing syndicalist outlook of groups like the railroad line committees represented a challenge that had to be faced head on.

Together with his socialist colleagues, Nekrasov soon developed what was, in effect, a comprehensive response based implicitly on the soviet principles that workers had a right to participate in industrial administration, that workers' control on the railroads was both necessary and desirable for effective line operations, that cooperation between workers and management was crucial to Russia's political and economic future, and that serious efforts had to be made to diminish unjust wage inequalities and improve material conditions. Though not articulated as such, Nekrasov's program (and, hence, government policy) was essentially comprised of three interrelated parts: what might be called the "statization" of railroad workers' organizations; the limitation of committee authority; and the equalization of railwaymen's wages.

First, Nekrasov pressed the idea that the railroads, as Russia's lifeline and most vital industry, had an importance extending far beyond normal bounds and had to be regarded as institutions of "state competence and significance." This justified his extending state regulations to all private lines in late March and early April and using the transport ministry's administrative apparatus to facilitate the formation of an all-Russian railroad union, whose leaders would work closely with ministerial personnel committed "to revolutionary democracy"—whose "official representative," he declared, "I consider myself."<sup>37</sup> Hence Nekrasov circulated through the ministry's own network all declarations and appeals of the Petrograd Soviet's railroad commission. He brought Soviet representatives into its various committees, a move he thought "both logical and necessary"; and, in early April, he began to make funds available to the organizers of an all-Russian railroad union, supporting a provisional conference and funding the operations of the "Provisional Union Executive Committee," the forerunner of *Vikzhel*. Thousands of rubles may have been spent for this pur-

<sup>36</sup> Z. Lozinskii, *Ekonomicheskaiia politika vremennogo pravitel'stva* (Leningrad, 1929), 26-32.

<sup>37</sup> *Volia i Dumy Zheleznodorozhnika*, May 14, 1917.



pose.<sup>38</sup> Despite attacks from other ministers and members of his own party, who accused him of “stabbing them in the back,” Nekrasov argued enthusiastically for the role an “autonomous state union” could play in railroad affairs. As he told a huge gathering of workers at the Bolshoi Theater in early May, railroaders would not follow traditional paths, since they were limited by law in terms of competence and activities. The All-Russian Railroad Union would be autonomous, “with state significance [*gosudarstvennoe znachenie*] acquired not because official state representatives participated . . . but because railroad workers themselves operate the railroads, which are the vital nerve of the whole country as well as national property, the property of the whole nation.”<sup>39</sup>

Second, Nekrasov moved to delineate clearly the limits of workers’ committees’ competence, insisting on the need for “strict revolutionary discipline.” In late May, he issued a special Circular Instruction 6321, comparable in the eyes of some observers to the Petrograd Soviet’s famous Army Order Number One.<sup>40</sup> Circular 6321 officially sanctioned existing railroad workers’ committees but only as individual units of the All-Russian National Railroad Union; specific powers were consequently left to the determination of that organization’s national congress, scheduled for late July. Meanwhile, line committees were to continue to have broad competence, but they could not issue instructions or orders unless specifically authorized by law, which included dismissing railroad administrators and electing their replacements. Hundreds of administrators had been discharged from the lines in the first months of the revolution, and Nekrasov’s circular was designed to end this process (although the minister himself felt that, up until May, these dismissals were justified in “ninety out of one hundred cases”).<sup>41</sup> The circular thus officially sanctioned the committees, but within a limited range of action.

Finally, in addition to “statization” and the joint legislation and limitation of committee authority, Nekrasov and soviet leaders moved together to equalize railroad wages and to relieve the hardship of lower-echelon employees. Since early March, the joint soviet-ministry commission under George Plekhanov had been working on a comprehensive solution to the wage issue, but in early May it decided that the situation of many railroaders had become desperate and that a series of emergency interim measures was necessary—and possible under the law. These involved, essentially, the distribution of a new “war bonus” of not less than one hundred rubles in districts with the highest cost of living and not less than forty rubles in the least expensive districts, heavily weighted in favor of lower-paid workers. The commission recognized that these funds were quite “inadequate” but still expected that they would “provide immediate assistance to the most needy sectors of the railroad work force.”<sup>42</sup> From some government

<sup>38</sup> Nekrasov mentioned fifteen thousand rubles, but the actual amount may have been much larger; see *Vestnik Putei Soobshcheniia*, 15 (April 15, 1917); and *Volia i Dumy Zheleznodorozhnika*, May 14, 1917.

<sup>39</sup> *Volia i Dumy Zheleznodorozhnika*, May 14, 1917.

<sup>40</sup> *Vestnik Putei Soobshcheniia*, 25 (June 24, 1917): 270.

<sup>41</sup> *Ibid.*, 22 (June 3, 1917): 111–12.

<sup>42</sup> *Volia i Dumy Zheleznodorozhnika*, May 26, 1917. Bonuses were to be distributed by local line administrations in agreement with central line committees, which reduced past bonus allotments to women by 15 percent and set limits of 25 to 50 percent on other categories of labor, particularly watchmen, employees entitled to railroad meals and lodging, teachers, and medical service personnel.



and liberal quarters, the Plekhanov bonus brought howls of complaint about pandering to worker militance and the weakening treasury; but, to Nekrasov, Plekhanov, and other supporters of a responsive regime, wage equalization and pay increases were a necessary adjunct to "statization" and the limitation of committee competence as a means of bringing the whole issue of mass organizations on the railroads under firm leadership and control.

WITH HINDSIGHT IT IS CLEAR that these policies, though necessary from the standpoint of regularizing railroad committee activities, maintaining productivity, and responding to worker grievances, also intensified the broader pressures of social polarization that in the early summer of 1917 began to rip Russia apart. As the regime launched its ill-fated June offensive, and then found itself beset by the Bolsheviks' attempt to seize power in July and the collapse of the first coalition, attacks in the liberal press on peasant and worker anarchism were accompanied by growing unease in government and soviet circles alike about the continued efficacy of workers' control at a time of dwindling state resources, disaster at the front, and the simple inability of Russia to meet its pressing economic needs. In scores of worker meetings and line congresses, events prompted new questions about "whose railroads" and "whose revolution" railroaders were supporting, but members of Russia's liberal and socialist intelligentsia found themselves increasingly pressured both toward greater support for state authority and toward still further restraints on committees and other forms of mass participation. This brought soviet and government figures closer together, lessening the consequences of dual power at the national level (particularly with the formation of the second coalition under Kerensky in July), but intensifying the sharpening division between the "democratic" commitments and goals of ordinary workers and peasants and those of most national leaders. It also engendered an increasingly strong sense of betrayal among those who thought in March and April that their national leaders stood firmly behind workers' control.

This polarization can clearly be seen on the railroads in the dramatically harder line toward committees taken after the publication of Circular 6321 by such groups as the State Council on Transport and the Association of Private Railroads as well as in the attitudes of government and soviet officials themselves. In late June, for example, as administrators on the Nikolaev line were meeting with the line's central committee to "resolve questions of their mutual cooperation," the vice minister of transport Takhtemyshev suddenly "electrified" the gathering with a scathing attack on the "outrages" of the committee, its "rapacious, malicious attitude," and its unwarranted demand for "self-government."<sup>43</sup> In the State Council on Transport, the railroads were described as in "total chaos," "covered with Bolshevik propaganda," "oblivious to all authority," and "bent on seizing power"—charges that were, at best, grossly exaggerated. Some of the recent dislocation on the lines, in fact, was the result of demobilizing soldiers commandeering trains or of various officials insisting they

<sup>43</sup> *Mysl' Zheleznodorozhnika*, July 1, 1917.

had priority in the allocation of equipment—problems that line committees, in most places, were struggling valiantly to control.<sup>44</sup> In any event, the basic causes of railroad dislocation antedated the revolution, as transport officials had themselves recognized in February.

Yet, despite these attacks and the growing problems and dangers of day-to-day railroad operations, railroaders as a whole generally remained “loyalist” in the summer of 1917, supporting the war in their congresses and meetings, condemning the July Days, still pledging support for the Petrograd Soviet and the coalition regime. In some places contributions for the government’s Liberty Loan were even deducted automatically from salaries and wages, with little or no protest.<sup>45</sup> Consequently, these broad patterns do not fully—or even primarily—provide an explanation for the transition from February to October on the railroads. Indeed, these developments suggest that railroaders might have supported to a greater extent the all-Russian union and Nekrasov’s policy of “statization” as well as the coalition regime and its minister of transport.

But the results of Nekrasov’s and the soviet leadership’s efforts weakened, rather than strengthened, railroader support. Each of their “solutions” provoked not social or political harmony but new antagonism and new difficulties. And, in coping with these new problems, commitments to “democratization” in the sense of mass participation and control became fundamentally incompatible with efforts to establish a stable democratic political order, while Bolshevik rule for tens of thousands of “loyalist” railroaders became an acceptable, if not entirely desirable, alternative.

Let us first examine the problem of wage equalization. The ministry’s policy here undoubtedly stemmed from a special concern about militant shop and depot workers. Wage levels for these positions were generally below those for comparable job categories in other industries. Shop workers were vocal in their demands for wage increases in March and April and repeatedly threatened to strike. Raising their wages, therefore, seemed politically prudent as well as compatible with socialist ideals.

Yet the basis on which the Plekhanov bonus was calculated remained the piece rate. With increasing shortages of supplies, the frequent absence of technicians and skilled workers (many of whom were off as delegates to conferences and meetings!), and the influx of returning workers from the spontaneously demobilizing army, new declines in shop productivity occurred, and the real gains from bonuses calculated on a piece-rate basis sharply decreased. Workers most favored by the bonus consequently remained dissatisfied. They demanded hourly or daily wages and an end to piece rates based on output. Even socialist officials like Plekhanov, however, could not support state interference in such a essential feature of shop administration as this without doing violence to principles of political democracy and the rule of law; now, as earlier, factory administrators remained convinced that the piece-rate system was the last best defense

<sup>44</sup> See especially *Novaia Zhizn'*, June 3, 1917; and the report of E. V. Landsberg to the Special Council on Transport, July 13, 1917, in *Ekonomicheskoe polozhenie*, 2: 238–42.

<sup>45</sup> *Vestnik Ekaterininskoi Zheleznoi Dorogi*, 510/34–36 (September 12–27, 1917).

against further disastrous declines in productivity. For those opposed to piece rates, therefore, the only alternative was to attempt to change fundamentally Russia's political and socioeconomic structure—in other words, to take over completely shop administration and to establish, in effect, a socialist order, which was precisely what the Bolsheviks wanted to do.

The very effort to equalize wages and reduce differentials, moreover, offended many skilled railroaders, who cherished their various petty privileges and perquisites. The engine drivers in particular reacted angrily to news that unskilled assistants would get relatively greater increases than themselves. Their reaction on some lines was to climb down from their cabs. Other workers and employees were also indignant, especially about efforts to ameliorate the conditions of road repair gangs, traditionally the lowest group in the railroad social hierarchy.<sup>46</sup> On this issue, given the importance of skilled, upper-echelon employees to railroad operations, the regime soon felt forced to promise a new series of bonuses for skilled labor categories; but, by mid-August, the situation had not been resolved, and a new series of brief but crippling strikes by drivers occurred. By pressing wage equalization, the left liberal-socialist leadership was thus pulling threads in the railroaders' rather closely woven social fabric, kinking one rank as another was tugged out of line. Hierarchy remained important among many workers, despite radical revolutionary commitments and deepening animosity toward privileged "bourgeois" society.

One consequence of this was to reinforce particularistic tendencies among the railroaders' multifarious occupational groups, prompting different groups of workers to begin organizing their own "union of conductors" or "union of locomotive engineers" to defend their special interests. These "ribbon unions," as they were called, had begun to proliferate like spring mushrooms in April and early May, much to the consternation of central line committees, ministry officials, and all-Russian union organizers alike. In early summer, with joint soviet-government efforts to equalize wages, they suddenly became a great deal more attractive as a means to satisfy special interests at precisely the moment when the regime was attempting to unite all railroaders in a single national union and bring order to committee activities by making them national union cells. And the popularity of the "ribbon unions" came, paradoxically, largely in response to sympathetic government actions.

A second result of these government efforts, however, was even more consequential. It centered on the question of limiting committee authority. In this instance, again paradoxically, although Plekhanov, Nekrasov, and others were convinced that the committees were still the best place to solve wage and productivity issues, the limitations placed on the committees' competence greatly affected their ability to do so. On the one hand, line administrators began immediately after the publication of Circular 6321 to by-pass the committees and reassert their personal authority. "Before 6321," a representative from the Ria-

<sup>46</sup> *Golos Zheleznodorozhnika*, July 2, 1917. Also see D. M. Zol'nikov, "Stachechnoe dvizhenie zheleznodorozhnika v 1917 g.," *Uchenye Zapiski Tomskogo Universiteta*, 38 (1961): 28-42.

zan-Urals line told the railroad union leaders in July, for example, "the administration worked closely with us and came to discuss important matters. After the instruction was issued, attitudes changed completely. Administrators began to refuse to meet with us and regarded us with hostility."<sup>47</sup> Here and elsewhere, *nachal'niki* reasserted sweeping powers, relying on past traditions and laws. Orders "had to be obeyed without the slightest reservation"; committee members were "forbidden absolutely to interfere in the issuing of line directives" and were permitted "only to express their personal views" under threat of dismissal.<sup>48</sup> On the Northern railroad, *nachal'niki* who had continued in their positions after the February Revolution with the express approval of the line committee now issued orders forbidding any criticism of the provisional regime, any opposition to the military offensive, or "the incitement of one group of the population against another." All violators were to be "prosecuted for treason."<sup>49</sup>

On the other hand, many railroad workers properly understood the central line committees' declining authority in the eyes of soviet and government officials and, after 6321, began themselves to by-pass their own organizations in pressing their complaints. An "enormous number" of persons began coming directly to the transport ministry with their problems, according to the ministry's socialist secretary, "ignoring the very basis of all-Russian railroad union organization." Thousands more sent letters and telegrams. By mid-July, almost eight thousand cables had been received from telegraph operators, asking the ministry directly about such individual matters as when they would "finally" be given a vacation.<sup>50</sup>

This circumvention of the line committees led, of course, to additional fragmentation among railroad workers and accelerated the growth of "ribbon unions," but it also pressed central line committees to assert their authority further—by insisting on new powers of dismissal and other sanctions, which they did not legally possess. Here the social position of most committee members came into play. Many clerks, engineers, and even jurists and medical personnel among committee members were deeply concerned about Russia's national interests and identified with the intelligentsia generally, rather than with ordinary workers. For them the question was not so much a reform in the nature of authority on the lines and in the workplace but a determination of who should exercise this authority in order to produce the highest level of transport efficiency. Yet such actions also pressed more militant local committees into demanding further "democratization."

If the goals of wage equalization and the delineation of committee authority were thus contradicted by consequent particularization and social antagonism on the lines, the third element of the "model" government-soviet approach—organization of an all-Russian railroad union with state or governmental significance (*gosudarstvennoe znachenie*)—generated still another range of problems. *Gosu-*

<sup>47</sup> *Biulleten' Vserossiiskogo Uchreditel'nogo Zheleznodorozhnogo S'ezda* (Moscow, 1917), 2 (July 29, 1917).

<sup>48</sup> *Vestnik Riazansko-Ural'skoi Zheleznoi Dorogi*, 23 (July 20, 1917).

<sup>49</sup> *Zheleznodorozhaia Zhizn' na Dal'nem Vostoke*, 27 (July 15, 1917).

<sup>50</sup> *Volia i Dumy Zheleznodorozhnika*, July 16, 1917.

*darstvennoe znachenie* was certainly not a clear expression. It appealed to syndicalist attitudes among railroad committeemen by implying a union with governmental powers or, at least, a union whose resolutions and decisions the government would implement into law with little discussion or change; and the organizers of the all-Russian railroad congress certainly understood it this way in March and April, when it was first used. But Nekrasov's speech at the Bolshoi Theater in early May made it clear that "statization" meant a good deal less than assigning a national railroad union full governmental powers. Since state competence derived from the fact that railroads were "the property of the entire people," Nekrasov claimed, railroad workers would acquire through their union "a special state significance that, on the one hand, will give their union special rights but, on the other, will impose special responsibilities . . . and the duty to fulfill special state tasks."<sup>51</sup> These "special rights," Nekrasov elaborated further, included "autonomy" and the determination of a broad range of welfare issues, such as wage rates, at the union's all-Russian congress, but the "special responsibilities" clearly meant obedience to government directives.

In many ways, the response of rank-and-file railroaders to these dubious advantages of "statization" was predictable. No substantial powers were actually granted to union leaders that other trade unions did not enjoy, while "special state responsibilities" implied that railroaders could not oppose government policies. And, indeed, Nekrasov's speech was met immediately by a strong rebuttal from S. V. Dvumiantsev, the prominent SR chairman of the Moscow-Kursk central line committee and a leading Moscow labor organizer: state significance without state power, he argued angrily, meant simply that the union would become a government instrument.<sup>52</sup> This view was shared not only by militant shop and depot workers but also by the staunchly loyalist union of railroad engineers and technicians, one of the few professional associations allowed to function under the tsar. The engineers agreed that railroaders had to subordinate their professional interests to those of the state but could not see how this could be done effectively without union leaders becoming, in effect, state officials—a view shared by many of the provisional *Vikzhel'* members themselves. "Statization" meant to them, in other words, that the railroad union should become a state agency, something the government could not and would not allow.<sup>53</sup>

Assigning "government significance" to the railroad union, moreover, led directly to two further problems: one concerning the extent to which the union leadership (in its social and occupational composition) was representative, and one concerning the government's own policies. The provisional executive committee elected at the all-Russian union's April conference was not, in fact, a broadly representative body, any more so than were many of the railroad line committees. Its fifteen members included only two grey- and three blue-collar workers; and its five-man presidium was composed of three jurists, one engineer, and one high-ranking administrative office worker. But, as P. Vompe of the

<sup>51</sup> *Ibid.*, May 1, 1917 (italics added).

<sup>52</sup> *Ibid.*

<sup>53</sup> *Ibid.*, June 10, 1917. Also see *Russkiiia Vedomosti*, June 18, 1917.



Nikolaev line argued in *Mysli Zheleznodorozhnika*, it behaved like a *khoziain* ("master"), sending telegrams and directives up and down the lines and acting like a governmental agency when, in fact, it was at best a consultative organ.<sup>54</sup> Indeed, in these circumstances various categories of railroaders could quite logically wonder whether their particular needs might better be served by ordinary trade unions not burdened with special state responsibilities. Similarly, the notion of subordinating railroad workers' interests to the state could quite understandably raise broad political questions. Minister Nekrasov's "solution" presumed an essential nonpartisanship in railroader outlooks, but this was no more realistic here than it was with the military; and, in fact, the government did not want a disinterested neutrality on the issues of the day, but active support.

Hence, "statization," which was also being pressed at this time by the socialist minister of labor Skobelev for other vital industries, necessarily injected politics into questions of union structure and organization, while it also greatly intensified already existing proclivities toward occupational professionalization. Discussions about the union's competence led to arguments about state policies and to the formation of Socialist Revolutionary, Menshevik, and other party caucuses in line congresses and committees—including, of course, caucuses dominated by Bolsheviks. On June 18, a conference of Socialist Revolutionary railroad workers convened in Moscow; in early July, other party organizations began to draft their own statements and proposals for presentation to the all-Russian congress.<sup>55</sup> Meanwhile, "ribbon unions" of telegraphers, shop and depot workers, engine drivers, conductors, and other railroad occupations expanded rapidly. The whole notion of an all-embracing national railroad union itself came under attack as these "sectarian" groups, despite competing interests, shared the conviction that all workers would best be served by professional unions in the traditional sense, with the government in the adversary role of "management." And, as line committee members and other all-Russian union delegates prepared for the first national railroad congress in July, the "professionalists" also gathered in their meetings and prepared to bring their case for aggressive shop unionism to the broader railroad work force.

THE EXTENDED GOVERNMENT CRISIS after the July Days and the collapse of the military offensive represented an important turning point in Russia's transition from February to October. Organized only after intensive negotiations among various leading social groups, the new coalition cabinet was committed both to re-establishing firm state authority and to retaining strong soviet support; but its socialist complexion and its commitment to soviet programs were hardly adequate grounds for building a strong popular base, and its concessions to those like General Kornilov, determined to restore military discipline and state authority, failed to restrain the deepening hostility of "bourgeois" and "privileged" Russia. Popular support for the Bolsheviks increased even as Trotsky and others

<sup>54</sup> *Mysli Zheleznodorozhnika*, June 21, 1917; and Taniev, *Ocherki dvizheniia*, 23–24.

<sup>55</sup> *Golos Zheleznodorozhnika*, June 25, 1917.



were arrested and Lenin went into hiding; many liberals and others temporized their commitments to political democracy and began seriously to consider alternative forms of state power.<sup>56</sup> While Kerensky's new cabinet thus became even less identified with the popular meaning of February, it also became increasingly unacceptable to those who legitimized the revolution in terms of Russia's national interests, and this, in part, because of its very coalition character. In a phrase, the sociopolitical foundations of the civil war were being set firmly in place.

In these circumstances, and against a background of increasing disruption and disorganization on the railroads, of potentially catastrophic declines in transport operations, and of growing worker demoralization, the nature of committee control on the railroads came under new challenge from masses of workers and "privileged" elements alike, one for failing to meet popular needs and the other for contributing to "anarchy" and "chaos." Nekrasov soon resigned. Plekhanov retired from the scene with an ambiguous statement insisting that the transport ministry would not "for one minute fail to meet the just demands of railroad laborers."<sup>57</sup> As late as August, Nekrasov's successor as minister of transport, P. P. Iurenev, continued to argue publicly that the desperate situation on the railroads was not of recent or "accidental" origin but the cumulative result of "deep organic difficulties" in the way that the railroads had been run for years.<sup>58</sup> Yet confidence in the process of democratization, badly eroded on all sides, was undermined, most importantly, among many railroaders themselves. "I am practically starving, comrades," a bookkeeper wrote in August to one railroad journal, for example:

I have exhausted all loans, and tomorrow will have nothing whatsoever even for a small scrap of bread. I won't discuss the causes of such conditions—they are known to all. I only want to point out that, as a bookkeeper, I am considered to be somewhat more privileged (at 150 rubles a month) than a majority of my fellow workers, and I am therefore not alone at the brink of starvation. . . . What is going to be done? I know very well that the government has gone through a ministerial crisis and that there is no chance of finding the means to increase my salary. . . . Yet all of us railroad workers desperately need food and fuel for the winter. I feel that catastrophe is upon us.<sup>59</sup>

Under such circumstances, "democracy" in the sense of mass participation undoubtedly retained its emotive content, as social polarization intensified in Russia at large, but its practical meaning either as a system of representative institutions or as a society committed to popular participation and welfare was increasingly unclear.

One dramatic consequence was the rapid increase in the process of professional sectarianism among railroad workers (*tsekhovshchina* ["shopism" or "craft

<sup>56</sup> For a discussion of this hostility and the changes in the liberals' position, see Rosenberg, *Liberals in the Russian Revolution*, 170–95, 203–33. On the growing strength of the Bolsheviks, especially see Rabinowitch, *The Bolsheviks Come to Power*, 83–93, 111–12.

<sup>57</sup> *Novaia Zhizn'*, June 22, 1917.

<sup>58</sup> *Biulleten' Vserossiiskogo Uchreditel'nogo Zheleznodorozhnogo S'ezda*, 11 (August 26, 1917).

<sup>59</sup> *Vestnik Ekaterinskoi Zheleznoi Dorogi*, 507/31 (August 21, 1917).

consciousness"] was the term some used), accentuating social differences between different labor groups and highlighting material inequities. The "pernicious lure of professionalism," as one commentator called it, which had earlier given rise to scores of "ribbon unions," now became intense.<sup>60</sup> Organizations of this sort grew quickly. The strongest were unions of shop and depot workers in Moscow and Petrograd, which expanded with close Bolshevik support. When the All-Russian Railroad Congress opened in mid-July, these workers even refused at first to participate; and they were active in the massive protest against Kerensky's State Conference in August. In September, they developed close contacts with the Bolshevik-dominated metal workers. Some saw this process resulting essentially from Bolshevik "subversion"; but the relationship between workers' control on the railroads and professional particularism and the growing disillusionment over the ability of a responsive and popular government to satisfy worker demands were far more important. In some cases, labor sectarianism did not even mean antagonism to important government goals, even among "Bolshevized" shop workers. In early September, for instance, a congress of shop workers on the Southern railroad insisted without qualification on workers' control over all areas of production "as the only way to pull Russia out of its current morass," but among the other demands of the one hundred or so delegates was also the "immediate introduction of universal obligatory labor [*trudovaia povinnost*]" as a means of increasing productivity.<sup>61</sup>

Other restricted and militant "ribbon unions" also expanded rapidly. Enrollment in *Madzhel'*, a new union of "lower-ranking railroad employees," increased dramatically in August.<sup>62</sup> Conductors and switchmen also organized, and they lodged strong protests with the Petrograd Soviet over the way national union leaders and government officials seemed to be ignoring their interests.<sup>63</sup> One of the most important and militant groups, seemingly disdainful of all authorities above them, remained the relatively well-paid engine drivers, the railroad "aristocrats," who strongly resented the government's efforts to reduce wage differentials. When a new bonus system was announced in August by the vice-minister of transport Ustrugov that again failed to meet engine-driver demands, the drivers threatened to strike all lines in the Moscow and Petrograd junctions.<sup>64</sup>

What appeared to many as political radicalism on the railroads was thus most of all an aggressive sectarian defense against the failure of "legitimate" authorities to protect diverse workers' interests. The response of many line committee and national union leaders in turn, however, was to emphasize the dan-

<sup>60</sup> *Volia i Dumy Zheleznodorozhnika*, May 5, 1917. Thus, the Union of Junior Agents of the Operations Service, *Madzhel'*, organized in late April, represented by mid-summer some forty-five thousand workers; other "ribbon unions" grew with equal rapidity. See V. D. Gurevich, "*Madzhel'*" (*Soiuz mladshikh agentov dvizheniia zheleznnykh dorog*) 1917-1919 gg. (Moscow, 1925); and Metel'kov, *Zheleznodorozhniki*, 119-34.

<sup>61</sup> *Vestnik Iuzhnykh Zheleznnykh Dorog*, 37 (September 11, 1917). On the shop workers, see especially *Volia i Dumy Zheleznodorozhnika*, May 5, 1917; Zol'nikov, "Stachechnoe dvizhenie," 31-42; and I. M. Pushkareva, "Vseobshchaia sentiabr'skaia stachka zheleznodorozhnikov v 1917," in *Rabochii klass i rabochee dvizhenie v Rossii v 1917* (Moscow, 1964), 181-90.

<sup>62</sup> Gurevich, "*Madzhel'*," 120.

<sup>63</sup> *Rabochaia Gazeta*, July 25, 1917.

<sup>64</sup> *Birzhevzia Vedomosti*, August 19, 1917.

ger of "Bolshevism" and political fragmentation, to insist more forcefully than ever that they themselves represented Russian state interests, and that the all-Russian union as a whole had to enjoy official state competence. "There can be no doubt that railroad workers are possessed in the main with a concern for state interests, with state-oriented rationality," one of the all-Russian union leaders, Malitskii, argued in support of this effort. The all-Russian union could not, therefore, be simply a professional association but "a state organ, having tasks of a public-legal character . . . and bearing full responsibility for the operation of the entire railroad industry not only in the interest of railroad employees but in the interests of the entire nation."<sup>65</sup> The shop and craft orientation of rank-and-file railroaders, in other words, was being countered by an equally forceful syndicalist outlook on the part of leading committeemen and union activists.

These differences sharply divided railroaders during the early sessions of the All-Russian Railroad Congress in July, particularly as a parade of government and soviet officials alike stressed the importance of maintaining the war effort and postponing welfare benefits; but, by the time the sessions closed in August and a new executive committee, *Vikzhel'*, had been elected, the predominant view favored giving the union state powers. Editorial writers in *Birzheviia Vedomosti*, *Rech'*, and the liberal press in general consequently saw the congress and *Vikzhel'* as a clear indication of the dangers of labor radicalism in 1917, the culmination of dangerous and destructive "anarcho-syndicalism." The railroads were cynically deemed an "independent republic."<sup>66</sup> In fact, however, what the congress epitomized was not a drive for labor independence so much as the problems and contradictions in the efforts of moderate socialists and left liberals like Plekhanov and Nekrasov to find a solution to the problem of revolutionary power in 1917, as well as to the disjunction between the "democratization" of Russian social institutions and the requirements of political democracy. *Vikzhel'* reflected essentially an instrument of the weakened coalition regime's last hopes to build some degree of institutional support among vital sectors of Russian labor rather than an aggressive new force of worker radicalism. Its base at the all-Russian congress was overwhelmingly the upper levels of the railroad labor hierarchy, where "persons with administrative authority" constituted 25.5 percent of the delegates; office workers, 21 percent; jurists, teachers, engineers, and technicians, 19 percent; and line and shop workers, only 15 percent. Most important, the congress also firmly rejected the "professional" or "craft" principle of union organization. It refused outright to seat delegates from the twelve "professional" unions demanding admission (and representing more than one-third of the work force), and it adopted a formula describing itself as a *pravochno-publichno* ("public-legal") institution with state responsibilities, essentially echoing the government's view. With "stormy applause," it rejected as "legend" the idea

<sup>65</sup> *Biulleten' Vserossiiskogo Uchreditel'nogo Zheleznodorozhnogo S'ezda*, 2 (July 29, 1917), 6 (August 11, 1917).

<sup>66</sup> See, for example, *Rech'*, August 19, 1917, September 19, 21, 1917; *Birzheviia Vedomosti*, August 19, 1917; and *Russkiiia Vedomosti*, September 23, 24, 1917.

that railroaders intended to by-pass the regime and take complete control of the lines, pledging full support to Kerensky's "government of national salvation."<sup>67</sup>

Yet in September, when it became apparent that Kerensky's government did not think it possible to implement the new wage scales adopted by the railroad congress and more militant employees at various levels increasingly threatened a nationwide strike, the syndicalist orientation of committee members and *Vikzhel* represented to many inside the government and out the complete collapse of state authority and national order. Again, *Vikzhel*'s efforts were overwhelmingly aimed at gaining control over the "pernicious lure of professionalism" in order to contain strikers, at "taking over the leadership of the strike movement" in order to prevent a total strike on the railroads, which might include the termination of military and food supply transport, and at "conducting any strike with minimum ill effects."<sup>68</sup> As an editorial in the newspaper *Volia i Dumy Zheleznodorozhnika*, the organ of the Moscow-Kursk workers' committee, argued, it made no sense for rank-and-file railroaders to struggle against state authority since the latter was, in effect, based in the all-Russian union: "to struggle with authorities established by the people is to struggle with ourselves, and that would be absurd."<sup>69</sup>

EXACTLY SUCH STATEMENTS, of course, became the central arguments of Bolshevik authorities after October as they repressed worker independence, and the irony here illustrates rather dramatically how the democratization of Russia's railroads brought advantages to Lenin's followers. Even without exploring the question in detail, how the Bolsheviks' coming to power gained a degree of acquiescence far greater than their actual level of support becomes understandable: by attacking union leaders for identifying with state interests, by castigating moderate central line committees for their close relations with government officials and for their "usurpation" of local committee powers, and, above all, by supporting the particularistic interests of diverse railroad labor groups, the Bolsheviks fostered dissatisfaction among all railroaders. Alone among party activists, Bolsheviks seemed well tuned to service and craft identities and seemed to understand better than others the importance of social hierarchy in the workplace. Leninist categorizations served well in identifying labor particularities. Their strongest base of support, of course, was among shop and

<sup>67</sup> *Biulleten' Vserossiiskogo Uchreditel'nogo Zheleznodorozhnogo S'ezda*, 11 (August 6, 1917), 13 (September 2, 1917); and *Golos Zheleznodorozhnika*, August 2, September 7, 1917. The exact political complexion of *Vikzhel* is unclear. The group was apparently almost evenly divided among left socialists (Left SRs, Bolsheviks, and Menshevik Internationalists), moderates (Mensheviks, SRs, and Popular Socialists), and nonparty people. See P. Vompe, *Tri goda revoliutsionnogo dvizhenia na zheleznykh dorogakh Rossiiskoi Sovetskoi Respubliki* (Moscow, 1920), 8-10; O. Piatnitskii, "Vikzhel' do, vo vremia, i posle oktiabr'skikh dnei," in *Put' k Oktabriu* (Moscow, 1923), 175-77; and W. Augustine, "Russia's Railwaymen," 668.

<sup>68</sup> *Izvestiia*, September 24, 1917. Here also, perhaps, are clues as to why such relatively little effort was made after October to equalize wages or abolish social or professional hierarchies in the workplace. The principles of Bolshevik union organization immediately after 1917 corresponded neatly with the workers' own craft and professional identities.

<sup>69</sup> *Volia i Dumy Zheleznodorozhnika*, July 26, 1917. For a discussion of the September strike and subsequent events, see Pethybridge, *The Spread of the Russian Revolution*, 40-47.

depot workers, but party activists on the lines were also apparently able to identify with *all* craft unions in one way or another. They supported the "aristocratic" engine drivers in their work stoppages and at the same time found strong support within *Madzhel'*, the union of lower-level service personnel, whom drivers treated so badly. Even the lowly road-repair workers were approached by Lenin's followers, just as they approached hostile peasants in the local *zemstvo* election campaigns.<sup>70</sup> These patterns, obviously, were not unique to the railroads, but railroad workers potentially had real power, providing they were organized and they supported firm leadership. Had *Vikzhel'* actually controlled the railroad "army" and been able to commit it to the defense of political democracy, Lenin's rule might well have been short lived.

*Vikzhel'* was, however, far weaker than it seemed because democratization on the railroads, as elsewhere, meant mass worker participation in industry affairs rather than commitment to a democratic state order or support for those, like Nekrasov, who became increasingly identified with state interests. Professional sectarianism, localism, and committee members' concern to protect their own authority from outside encroachment engendered a growing disillusionment both with the soviet-liberal coalition regime, whose commitments to political democracy seemed increasingly irrelevant in terms of defending worker interests, and with the all-Russian union, which insisted it was a responsible state organ and struggled for its own "state competence." The expectations for material betterment that accompanied democratization were not, and probably could not, be met. When neither the government nor the union proved able to meet worker goals, when both attempted to restrain lower committee authority, and when Nekrasov, Plekhanov, and other responsive officials pressed wage equalization at the expense of traditional hierarchies and wage differentials, disillusionment turned to increased institutional fragmentation. On a number of railroads, particularly on the important Moscow-Kursk, Moscow-Kazan, and Nikolaev lines, where the influence of shop workers was quite strong, central line committees reacted to these developments by assuming even more authority on the eve of the Bolshevik coup, as institutional autonomy remained a last defense against unacceptable outside allegiances. (Shortly after the Bolsheviks came to power, in fact, the Nikolaev line became the first railroad in history to be fully managed by its own worker representatives.) Over these groups, without question, *Vikzhel'* remained powerless to implement any directives that were not specifically acceptable to the railroaders themselves.

The all-Russian union leadership was thus organically weak, despite the apparent radicalism of its syndicalist mentality and however otherwise it might have appeared to Lenin himself in October or to later historians and commentators. The solutions to the problem of revolutionary political power sought by Nekrasov and moderate soviet figures, based on close government-labor relations, therefore contained inherent frailties. Support for the process of democratization clearly brought little backing or political strength, and no gratitude.

<sup>70</sup> Gurevich, "*Madzhel'*," 16-18; and Metel'kov, *Zheleznodorozhniki v revoliutsii*, 119-33.



What was surprising about Circular 6321, in fact, was not the opposition of those who felt it violated the premises of civil liberties and a rule of law but the anger of railroaders themselves, whose organizations and mass involvement it officially legitimized. Nekrasov unsuccessfully faced the problem of eliminating authoritarianism from the workplace without disturbing social and professional hierarchies, a difficult task in the best of circumstances. But, more important, the opposition of many workers to what union leaders and many committeemen themselves recognized as reasonable and appropriate restraints on mass action showed there was increasingly little correlation in 1917 between mass participation, economic decentralization, popular psychology, and the procedures and institutions of democratic rule. So strong were the internal contradictions of Nekrasov's solutions, in fact, that political democracy might not have been able to survive in revolutionary Russia even under a democratic socialist regime; the logical outcome seems necessarily to have been one or another form of political authoritarianism, at least for the immediate future.

Finally, perhaps, prevailing conceptualizations of democracy delimited its potential for success in the Russian revolution, creating the dichotomy between the dynamics of political difficulties at high levels and the achievement of "democratization" below. The experience of Russia's railroaders makes it clear how the Bolsheviks could, in the end, appropriate the emotive and psychological content of "democracy" as it was popularly understood, pledge themselves to defend proletarian class interests while acknowledging professional and craft differentiations, identify class and state interests, and be accepted even by non-Bolshevik workers as "democrats." In this sector, as in others, Russia's experience with democracy was itself prelude to the Soviet state.

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## The Urban South: A Regional Framework

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DAVID R. GOLDFIELD

A SHORT TEN-MINUTE DRIVE from downtown Atlanta and you are no longer in the city; you are in the South. There it is still possible, journalist Pat Watters wrote, "of a late spring evening . . . to breathe the air of a small-town America (not suburbia) of the American past, suffused with the coolness and blossom fragrance of trees and bushes, roses, honeysuckle, and the wet smell of grass and weeds."<sup>1</sup> The aroma had barely subsided when historians began to draw a different portrait of the urban South, demonstrating that, over the past two centuries, urban Southerners were concerned about the same issues and problems as residents in cities elsewhere and were as aggressively capitalistic in pursuing growth and prosperity. If the urban South lagged behind the urban North in population and production, the lag was of time, not of quality. Southern cities, in short, were full-fledged members of the urban nation, distinguished only by latitude and pace.<sup>2</sup>

Such historical studies—and the complementary works of sociologists, geographers, and political scientists—had the beneficial effect of lifting the "cotton curtain" that had shrouded an important aspect of Southern regional development. The South could no longer be thought of simply as "the plantation, the planter, the staple crop, and the Negro, all set in a rural scene."<sup>3</sup> The discovery of the Southern city, moreover, had significance beyond the academy. It provided a new perspective on an old region and portended well for its future. As

I am indebted to the suggestions of Leonard P. Curry, Don H. Doyle, and Howard N. Rabinowitz, who read earlier versions of this essay. The religious guidance provided by David E. Harrell and the sources recommended by my Stockholm University colleagues, Thomas Hall and Ingrid Hammarström, informed me and the article. David R. Godschalk graciously offered the resources of his Department of City and Regional Planning at the University of North Carolina, Chapel Hill, during the early stages of the article's preparation in 1979.

<sup>1</sup> Watters, *The South and the Nation* (New York, 1969), 138.

<sup>2</sup> See, for example, Blaine A. Brownell, *The Urban Ethos in the South, 1920-1930* (Baton Rouge, 1975), and Brownell and David R. Goldfield, "Southern Urban History," in Brownell and Goldfield, eds., *The City in Southern History: The Growth of Urban Civilization in the South* (Port Washington, N.Y., 1977), 5-22; Leonard P. Curry, "Urbanization and Urbanism in the Old South: A Comparative View," *Journal of Southern History* [hereafter, *JSH*], 40 (1974): 43-60; Lyle W. Dorsett and Arthur H. Shaffer, "Was the Antebellum South Anti-Urban? A Suggestion," *ibid.*, 38 (1972): 93-100; and Richard J. Hopkins, "Are Southern Cities Unique? Persistence as a Clue," *Mississippi Quarterly*, 26 (1973): 121-41.

<sup>3</sup> David L. Smiley, "The Quest for a Central Theme in Southern History," *Southern Atlantic Quarterly*, 71 (1972): 318.

early as the 1930s, Howard W. Odum and the Regionalists at Chapel Hill contended that urbanization would result in a cultural maturation that would alter the region's values. Southern political scientist V. O. Key asserted that "urbanization contained the seeds of political revolution in the South." Now cities were in a position to provide regional political leadership, which meant an end to the "tradition-bound" politics of the agrarian past.<sup>4</sup>

In the 1950s, urban analyst Robert Earl Garren predicted that urbanism as a "new way of life for the South" would solve "difficulties now present in human relationships"—that is, race relations. During the 1960s and 1970s, when the sunbelt hoopla coincided with increased scholarly interest in the urban South, enthusiasm over the region's future abounded. In the mid-1960s, sociologist Edgar T. Thompson averred that "the city everywhere is the natural habitat of the liberal mind, and the southern liberal is increasing in number and making himself heard"; liberalism would bring racial peace and political harmony to the region. Another sociologist, Leonard Reissman, declared at the same time that urbanization was destroying the social homogeneity of the South. The solid South was not so any longer. A decade later, sociologists Thomas H. Naylor and James Clotfelter agreed that urban growth was "undermining the unity of the region."<sup>5</sup> Scholars had clearly written an epitaph for Dixie.

They had also miscalculated. These scholars assumed the existence of a community of cities—that is, American cities sharing certain characteristics that made them more like each other than like their nonurban surroundings.<sup>6</sup> The assumption is based upon an urban model designed by Chicago sociologist Louis Wirth in the 1930s. In his seminal essay, "Urbanism as a Way of Life," Wirth contended that the city is a distinctive environment, set apart from the countryside and capable of altering human behavior by the very fact that it is a city.<sup>7</sup> For an increasing number of social scientists, however, it is becoming evident that this model is "both time- and culture-bound to the immigrant city of North America at the turn of the twentieth century."<sup>8</sup> Although the divergence of pluralistic Chicago from the homogeneous Illinois cornfields provided an empirical justification for Wirth and his followers, the dissociation of city and region has been less apparent to later scholars studying other urban settings.

<sup>4</sup> For this evaluation of Key's position, see James C. Cobb, "Urbanization and the Changing South: A Review of the Literature," *South Atlantic Urban Studies*, 1 (1977): 255. For an excellent discussion of the Regionalists' perception of urbanization, see Don H. Doyle, "Urbanization and Southern Culture: Economic Elites in Four New South Cities, 1865-1910," paper presented at the Forty-Fourth Annual Meeting of the Southern Historical Association, held in St. Louis, November 1978.

<sup>5</sup> Garren, "Urbanism: A New Way of Life for the South," *Mississippi Quarterly*, 10 (1957): 68; Thompson, "The South in Old and New Contexts," in John C. McKinney and Thompson, eds., *The South in Continuity and Change* (Durham, N.C., 1965), 478; Reissman, "Urbanization in the South," *ibid.*, 87; and Naylor and Clotfelter, *Strategies for Change in the South* (Chapel Hill, 1975), 222.

<sup>6</sup> For two different expressions of the view that urbanization mutes regional distinctions, see David Harvey, *Social Justice and the City* (Baltimore, 1973), 309; and Richard C. Wade, "An Agenda for Urban History," in George A. Billias and Gerald N. Grob, eds., *American History: Retrospect and Prospect* (New York, 1971), 393.

<sup>7</sup> Wirth, "Urbanism as a Way of Life," *American Journal of Sociology*, 44 (1938): 1-24. Actually, Wirth began to formulate and articulate these ideas at least a decade earlier; see, for example, his *The Ghetto* (Chicago, 1928).

<sup>8</sup> Brian J. L. Berry and John D. Kasarda, *Contemporary Urban Ecology* (New York, 1977), 376.

Accordingly, the growing propensity in the social sciences has been to study cities in the context of regions, under the assumption that regional and urban characteristics interact, influencing the development of both.<sup>9</sup> The flexibility of the regional model is one of its major advantages. It does not attribute a specific set of characteristics to either city or region but, instead, views the city as one environment among many in a given region. The concept of region itself is flexible in that a region may consist of the immediate hinterland, a nation, or even several nations. Canadian urban historian Gilbert A. Stelter has argued, for example, that fully to "appreciate any differences that might exist between Canadian and American cities" requires scholars to "go beyond urban development and examine the extent to which the Canadian experience differs from that of the United States." Context is the key to urban analysis under the regional model. As geographer Brian J. L. Berry has asserted, "one cannot study the ecology of an urban area in isolation."<sup>10</sup>

This essay proposes a regional framework for the study of the urban South. Applying Stelter's suggestion to the South, elements of regional distinction provide insights on how Southern cities differed from their counterparts elsewhere. Assuming that the South qualifies as a region, isolating factors of regional distinction is a necessary first step to analyzing their impact on regional and, especially for the purposes of this analysis, urban growth.<sup>11</sup> At least three general factors have characterized the South's historical development: ruralism, race, and colonialism. These elements have appeared in other American regions, but, in terms of their combination and continuity, they are distinctive to the Southern region and, therefore, to the Southern city.

<sup>9</sup> For some examples of regional studies in a historical context, see Michael P. Conzen, *Frontier Farming in an Urban Shadow: The Influence of Madison's Proximity on the Agricultural Development of Blooming Grove, Wisconsin* (Madison, Wis., 1971); Robert R. Dykstra, *Cattle Towns* (New York, 1968); and Diane Lindstrom, *Economic Development in the Philadelphia Region, 1810-1850* (New York, 1978).

<sup>10</sup> Bruce M. Stave, "A Conversation with Gilbert A. Stelter: Urban History in Canada," *Journal of Urban History*, 6 (1980): 181; and Berry and Kasarda, *Contemporary Urban Ecology*, 85.

<sup>11</sup> I make two assumptions here: first, that the South qualifies as a region and, second, that the Southern region is distinctive. There is a problem in treating the South as a geographic and statistical entity. It simply does not fit the region. I agree with David L. Smiley that the "South defies . . . location"; "The Quest for a Central Theme in Southern History." It is more a culture, as historian I. A. Newby has suggested, than a geography; see Newby, *The South: A History* (New York, 1978). Terms like "Piedmont," "Tidewater," "valley," "coastal plain," "piney woods," "Delta," "black belt," "low country," "upcountry," and "mountain" describe the various geographic divisions of the South and, doubtless, the peculiarities of life that exist within these segments as well; thus, there is no single geographic South. But geography is both too precise a tool to define the region and not fine enough. My own precise, yet imprecise, geographic definition of the South includes the eleven Confederate states plus Kentucky. Within this area the regional characteristics had their greatest impact. (The exclusion of Baltimore, St. Louis, and Washington, D.C., does not alter the character of Southern urbanization; it may move some statistical analyses a few percentage points up or down, but my study does not trace the development of Southern cities through population figures. Size is, of course, important, but only as an indicator of the general character of Southern urbanization.) The search for Southern distinctiveness, wherever its "location," has become a major regional theme in itself. All American regions originated as agricultural areas; biracialism is a national phenomenon; and other regions have submitted to the Northeastern economic juggernaut. Thus, cities throughout the nation most likely share certain characteristics because their regions have shared aspects common to American life in general. I argue, however, that the continuity and combination of these characteristics were unique to the South. Consequently, according to the regional framework of urban analysis, Southern cities were distinctive as well.

THE WIRTHIAN MODEL OF URBANIZATION established "urban" and "rural" as dichotomous environments. Contemporary scholars, however, are less certain of the differences, and a few have asserted that what distinctions exist are, in Brian J. L. Berry's word, "meaningless." Patterns of American postwar urbanization support this view. According to Oscar Handlin, "the differences between city and country have been attenuated almost to the vanishing point."<sup>12</sup> But, even in the industrial era at the turn of the century when urban-rural distinctions were perhaps more obvious, social scientists groped for definitional demarcations. Pioneer urbanist Adna F. Weber complained that, in the United States, the "town" is a rural concept but that, in Europe, it is considered urban and that the numerical thresholds assigned to each concept were inadequate. Weber was most satisfied with the German subdivision of *Landstadt* (literally, "country town") as a definitional middle ground between rural and urban places that combines elements of both.<sup>13</sup>

Indeed, Europeans in general have perceived the interchange between city and country characteristics. In the 1950s, Swedish urban historian Gregor Paulsson wrote that "alla städer ligger på landet" ("all cities are in the countryside"), referring to the rural manifestations evident in Swedish urban life. Similarly, French urbanist Henri Lefebvre contended that "urbanization of the countryside involves a subsidiary ruralization of the city."<sup>14</sup> European urban history is replete with examples to support the views of both Paulsson and Lefebvre.

The medieval European city included farms and orchards. Paris's Left Bank

<sup>12</sup> Berry and Kasarda, *Contemporary Urban Ecology*, 174; and Handlin, "The Modern City as a Field of Study," in Handlin and John Burchard, eds., *The Historian and the City* (Cambridge, Mass., 1963), 24, as quoted in Berry and Kasarda, *Contemporary Urban Ecology*, 254. The rapidly disappearing distinctions between urban and rural increase the definitional problems surrounding the question of what is—and what is not—a city. Scholars typically use size or function indicators to measure "urbanism," but they do not agree as to what particular thresholds comprise the designation "urban." See Gilbert Rozman, "Urban Networks and Historical Stages," *Journal of Interdisciplinary History*, 9 (1978): 65–91; and Ronald C. Tobey, "How Urbane is the Urbanite? An Historical Model of the Urban Hierarchy and the Social Motivation of Service Classes," *Historical Methods Newsletter*, 7 (1974): 259–75. The population thresholds devised by the U.S. Census Bureau are inadequate since they reveal little about a particular environment's function and influence. In addition, the relative population, function, and influence thresholds that distinguish towns and cities must change over time. To compound the definitional problems, an urban place can be a "city" in terms of possessing a relatively large number of inhabitants but a "town" in its functional characteristics—that is, performing basic agricultural marketing activities. It is important, nevertheless, to note some distinctions among various urban environments. My own solution has been to assign definitional priority to population levels and secondary emphasis to function and influence. Thus, I consider an urban place a "town" if it possessed fewer than one thousand inhabitants in 1820, fifteen hundred in 1850, twenty-five hundred in 1880, five thousand in 1910, seventy-five hundred in 1940, and ten thousand in 1970. "Town" functions are basically agricultural marketing, and "town" influence rarely extends beyond the immediate hinterland. When I use the adjective "small" to describe a city, I am referring to those urban places with populations of one thousand to twenty-five hundred in 1820, fifteen hundred to four thousand in 1850, twenty-five hundred to ten thousand in 1880, five thousand to fifteen thousand in 1910, seventy-five hundred to twenty-five thousand in 1940, and ten thousand to forty thousand in 1970. Their functions typically include some wholesaling and, perhaps, basic processing industries, and their market influence extends beyond the adjacent hinterland but not throughout the region. These definitions are merely guidelines and are not meant to be descriptive beyond the rudimentary characteristics presented here. I appreciate the assistance of University of North Carolina geographers Clyde E. Browning and Richard Garrity in developing these definitions.

<sup>13</sup> Weber, *The Growth of Cities in the Nineteenth Century* (New York, 1899), 14–16.

<sup>14</sup> Paulsson, *Svensk stad*, 1 (Stockholm, 1950): 6; and, for this statement of Lefebvre's views of urbanization in *La Revolution urbaine* (1970) and *La Pensée Marxiste et la ville* (1972), see Harvey, *Social Justice and the City*, 308.



was "semi-urban" with sprawling vineyards as late as 1200. Seventeenth-century London was a "pleasant country town with many gardens and broad green fields." And, even after the Industrial Revolution, agriculture remained a part of urban life. English urban historians H. J. Dyos and Michael Wolff described the Victorian city as still tied to its "rural connections. The largest of them still conducted extensive backyard agriculture, . . . cowstalls, sheep-folds, pig-sties above and below ground, in and out of dwellings, on and off the streets, wherever this rudimentary factory-farming could be made to work."<sup>15</sup> Cities that have lost their rural connections are of relatively recent origin and confined only to a highly industrialized segment of the United States and Western Europe. As late as the 1920s in China, market garden farms and duck ponds were typical features of the larger cities.

Life in Southern cities in the nineteenth century remained tied to agricultural cycles. There was, as Lewis Mumford noted of Greek cities, "a tidal drifting in and out of the city with the seasons." Towns slept from late spring to early fall and awoke with the arrival of the first cotton or tobacco shipments. Even a metropolis such as New Orleans did not have a life of its own apart from the dictates of the cotton fields. "About the first of June," journalist J. D. B. De Bow wrote with some dismay, New Orleans "begins to show evidences of waste. People inquire of steam and rail routes and are buying trunks."<sup>16</sup> Many were also escaping the potential of yellow fever; but, had the city been healthy, there was simply little to do while cotton was in the fields. The tempo picked up in the fall and especially in the early winter. Cities arranged their yearly social and cultural calendars around staple marketing time.

Southern urban architecture and landscape further reflected agrarian connections. Antebellum planters moved readily between country and city residences in larger cities like Charleston and established themselves more or less permanently in smaller places like Natchez and Demopolis; their presence contributed to the Southern city's hybrid appearance. Their homes, turned sideways to show narrow frontage on the street, had verandahs along the side and verdant gardens in the back to lend a country atmosphere. Travelers commenting on the distinctions between Northern and Southern cities frequently lapsed into rural metaphors. Palmettos and magnolias graced the streets of Charleston, while a walk on the Battery "to inhale the pure and cool breezes . . . and to enjoy the view" was an essential part of the itinerary of any visitor. Savannah, one visitor wrote to his Northern friends, "is a city of trees and gardens." Away from the wharf, this traveler noted an "almost rural quiet." Even on the Texas frontier, Galveston was "one of the most charming places—in appearance—that I have ever seen," one traveler exclaimed; the landscaping and the

<sup>15</sup> Yi-Fu Tuan, "The City: Its Distance from Nature," *Geographical Review*, 68 (1978): 3; and Dyos and Wolff, "The Way We Live Now," in Dyos and Wolff, eds., *The Victorian City: Images and Reality*, 2 (London, 1973): 899, as quoted in Tuan, "The City," 3–4. Tuan has presented an excellent discussion of agricultural activities in Western and Oriental cities throughout history.

<sup>16</sup> Mumford, *The City in History* (New York, 1961), 128; and De Bow, "Editorial Miscellany," *De Bow's Review*, 27 (1859): 117.

elegance of the residences along the Strand bespoke the same pastoral quietude of more civilized Natchez and Savannah.<sup>17</sup>

Southern cities continued to attract notice as middle landscapes into the twentieth century. Novelist Sherwood Anderson, absorbing the exotic culture of New Orleans during the 1920s, was most impressed not by the city's urbanity but by its close relationship to nature. In a letter to his publisher concerning the impending appearance of *Dark Laughter*, a novel he wrote in New Orleans, Anderson related, "The Negro, the earth, and the river—that suggests the title." The narrator in Anderson's "A Meeting South" stated, "All good New Orleanians go to look at the Mississippi, at least once a day."<sup>18</sup> This urban commune with nature, which struck Anderson as so unusual, had been the Southern urban condition for two centuries. Edd W. Parks summarized the relationship in 1934 by stating that the urban South was "governed and given character by the country immediately surrounding it."<sup>19</sup>

The relatively low density of population in Southern cities complemented the rural physical appearance. Because many Southern cities experienced their most rapid growth during the twentieth century, the age of annexation and the automobile, most of them remain uncluttered and sprawling. But the horizontal structure of Southern urban space was evident in the nineteenth century, long before technology affected land use. Single family dwellings, no matter how modest, characterized residential areas. Blacks and poor whites on the urban periphery merely re-created their small wooden rural shacks. The ample gardens of the larger homes lent an air of spaciousness to more affluent residential districts closer to the city center.<sup>20</sup> The low-rise aspect of residences persisted after local boosters began building commercial towers in refurbished downtowns at the turn of the century.

Rural elements not only permeated the physical structure of Southern cities but determined the very existence of those cities as well. Agriculture, especially staple agriculture, molded the region's economy and directed the size and nature of urban growth. Geographer Carville Earle and historian Ronald Hoffman have demonstrated how particular staple crops induced a specific pattern of Southern urbanization in the colonial era. Geography and the marketing practices of tobacco farmers limited urban development in the Chesapeake colonies, and shipping was the primary role towns performed. Though numerous, they were consequently small. When wheat cultivation engaged part of the area in the 1740s, larger settlements like Baltimore evolved. The processing and sub-

<sup>17</sup> Jane M. Turnbull and Marion T. Turnbull, *American Photographs*, 2 (London, 1860): 92, as quoted in Ivan D. Steen, "Charleston in the 1850's: As Described by British Travelers," *South Carolina Historical Magazine*, 71 (1970): 38; and Curtis B. Pyle, "Letters from the South," *Masonic Mirrors and American Keystone*, 2 (1853): 115, 125–26, and [Samuel Adams Hammett] "Drafts at Sight in the Southwest," *Literary World*, 5 (1849): 21–22, 217–18, both as quoted in Eugene L. Schwab, ed., *Travels in the Old South*, 2 (Lexington, Ky., 1973): 527, 430.

<sup>18</sup> Anderson, *Letters of Sherwood Anderson*, ed. Howard Mumford Jones and Walter B. Rideout (Boston, 1953), 142, and "A Meeting South," in *The Portable Sherwood Anderson*, ed. Horace Gregory (New York, 1956), 522, as quoted in Michael Fanning, "New Orleans and Sherwood Anderson," *Southern Studies*, 17 (1978): 206.

<sup>19</sup> Parks, "Southern Towns and Cities," in William T. Couch, ed., *Culture in the South* (Chapel Hill, 1934), 512, as quoted in Brownell, *The Urban Ethos in the South*, 6.

<sup>20</sup> See Constance M. Green, *American Cities in the Growth of the Nation* (New York, 1965), 24; and Mary Fulton Green, "A Profile of Columbia in 1850," *South Carolina Historical Magazine*, 70 (1969): 106.

sequent storage requirements of the crop stimulated the growth of larger urban places. Rice, like wheat, required extensive marketing, storage, and processing facilities. Charleston became the thriving center of rice cultivation in the eighteenth century.<sup>21</sup> The Carolina port also grew as a result of the slave trade, but that, too, was staple-inspired. Ira Berlin has demonstrated that profits from staples encouraged consolidation of landholdings, which in turn created huge labor demands.<sup>22</sup>

Urban growth, or lack thereof, during the colonial era is indicative of the influence of staple agriculture. But not until the antebellum period did a distinctive Southern urban system take root. With the emergence of cotton as the region's leading staple, New Orleans developed into the South's and, for a time in the 1830s, the nation's leading export center. In the cotton belt in general, however, urban civilization barely existed. The rural quietude of Natchez was matched by dozens of other Delta towns. Except for Mobile, New Orleans seemed to enjoy an urban monopoly. Of the ten leading Southern cities in 1850, only Memphis, Mobile, and New Orleans were in the cotton belt.

Cotton, like tobacco, required relatively few concentrated services. Processing occurred elsewhere, in New England or Great Britain, so the only requisite for the crop's commercial success was an outlet to both of these locations. New Orleans merchants organized the trade by sending agents into upriver towns to collect the crop and, in turn, supply the planters with wares. The volume of cotton production and the need for quality control and price stability militated against the kind of diffuse trade that marked tobacco commerce in the early colonial era. The functions of upriver towns were limited to collection points for the staple. New Orleans's early regional dominance of cotton commerce, moreover, inhibited urban growth by restricting capital accumulation. Crescent City merchants came to control local banks as well. Finally, as the regional center for the slave trade, New Orleans had another economic monopoly to drain capital from the hinterland. So, cotton and capital flowed to the delta port.

Soil exhaustion and the limited marketing procedures surrounding tobacco cultivation restricted the development of cities similar in size to those in the North. The Chesapeake (not including Baltimore, which was tied to wheat-cultivating areas) and the North Carolina tobacco belt provided little sustenance for Richmond, Lynchburg, and Petersburg. Only in the 1850s, when these cities added processing industries, did growth occur, though on a modest scale. Thus, New Orleans was the dominant metropolis of the antebellum South; it had no rivals. Charleston, its nearest competitor in population, was one-quarter its size in 1860 and had slipped from sixth to fifteenth in national rank according to size in less than a generation.

Urbanization did indeed occur in the antebellum South. What did not, however, was the development of large cities. An urban place inhabited by fewer

<sup>21</sup> Earle and Hoffman, "The Urban South: The First Two Centuries," in Brownell and Goldfield, *The City in Southern History*, 23-51.

<sup>22</sup> Berlin, "Time, Space, and the Evolution of Afro-American Society on British Mainland North America," *AHR*, 85 (1980): 58-61.

than four thousand persons (by 1860) was more characteristic of the antebellum South than of any other region. The urban population of the region was, therefore, more diffuse—a condition consistent with the relatively few economic functions such cities performed in support of a staple crop economy. It was urbanization without cities.<sup>23</sup>

The pattern persisted for nearly another century. The development of the Southern urban system following the Civil War reflected the South's increased dependence on staple agriculture. During the period of agricultural recovery, 1865–80, urbanization came to a virtual halt. The percentage of the South's population residing in cities increased from 9.6 percent in 1860 to only 12.2 percent by 1880. In the highly urbanized Northeast, by contrast, city dwellers increased from 35.7 to 50.8 percent of the population during the same period. The ground lost by the laggard pace of Southern urbanization immediately before the Civil War was never recovered. In 1830, five Southern cities were among the nation's twenty leading cities; in 1900, only one—New Orleans—remained, and only six Southern cities were among the fifty major cities in the United States.

Changes in both marketing and processing cotton led to the even greater prevalence of smaller urban places in the Southern urban system than had existed in the antebellum period. The appearance of country stores, storehouses, and taverns around rural railroad stations signified a localization of cotton marketing. Normally, this system should have benefited Southern port cities, which could be expected to supply these merchant-crossroad settlements. The extent of the national railroad network, however, enabled these storekeepers to by-pass Southern cities in favor of connections with Northern cities. In this way, the country merchants could market the local cotton crop in exchange for goods without requiring contact with Southern ports.

The precipitous decline of New Orleans reflected the new marketing arrangements. New Orleans remained the major Southern cotton port, but only because the lower Mississippi provided easier transportation for some planters than did the railroads running eastward and westward; and not until 1883 did cotton receipts at the port attain prewar levels. Population growth, moreover, declined with commerce: in 1860, New Orleans was the sixth largest city in the nation; by 1900, it had slipped to fifteenth and was continuing to move downward.

In addition to marketing changes, the improvement of staple processing techniques in the late-nineteenth-century South induced further decline in the region's major ports. Technological innovations that enabled cotton gins to process more cotton encouraged the removal of ginning from individual plantations to nearby towns along rail routes where cotton could be cleaned more cheaply in greater volume. By the 1880s, two new processing techniques had appeared—

<sup>23</sup> On the antebellum Southern urban system, see Clement Eaton, *The Growth of Southern Civilization, 1790–1860* (New York, 1961), 248–50; Curry, "Urbanization and Urbanism in the Old South," 43–55; David R. Goldfield, "Pursuing the American Urban Dream: Cities in the Old South," in Brownell and Goldfield, *The City in Southern History*, 56–57; and Rupert B. Vance and Sara Smith, "Metropolitan Dominance and Integration," in Vance and Nicholas J. Demerath, eds., *The Urban South* (Chapel Hill, 1954), 120.

cotton compressing, which reduced the size of the bales, and cottonseed oil mills, which extracted oil from crushed cottonseeds. Both of these techniques required relatively sophisticated machinery, which could handle the product of roughly thirty cotton gins at one time. Such industries, therefore, commanded a wider market area than the ginning enterprises and, consequently, produced urban growth. Until 1930, those communities that were able to secure all three processing services were the fastest growing cities in the cotton belt. These additional services left the planter even less reason to patronize the once-flourishing seaports.

Although the processing communities were major beneficiaries of cotton belt urban growth, they attained the limits of their expansion relatively quickly. By 1900, these towns, which were scattered remarkably evenly throughout the area of cotton cultivation from Texas to southern Georgia, had reached a population of five to ten thousand inhabitants and rarely grew beyond that. The market area required by the new procedures remained relatively stationary, and, since the processed cotton was immediately transshipped by rail to major rail centers in the South like Dallas and Atlanta or to Northern cities, the towns had little need to develop higher economic functions. Actually, those cities that were outside the cotton belt or had very little contact with the staple were most successful in generating large-scale urban growth. Cotton cultivation, as in the antebellum era, could produce a significant number of urban places, but their size was severely restricted by the limits of the marketing and processing activities imposed by the crop.

As staple cultivation came to characterize the postwar South to an even greater extent than it did in the antebellum era, the small city became even more typical of Southern urban settlement. In 1850, 68.7 percent of the South's urban population lived in cities of over twenty-five thousand inhabitants; by 1900, only 48.1 percent did, even though more Southerners lived in cities by the later date. Cities that served as major transshipment points for cotton or had little direct contact with the staple became the region's new growth centers. Among the South's five most populous cities in 1920, Atlanta possessed only a few hundred citizens in 1850, and Birmingham did not exist at that date. But Atlanta and Birmingham were only the most prominent examples of urbanization beyond simple marketing and processing. The appearance of cities like Durham, Winston-Salem, and Greenville in the Carolina Piedmont reflected a more complex industrial base than simple processing. The rise of Florida cities such as Jacksonville and Tampa, which grew from a mixture of commerce and industry, and the growth of Chattanooga, Knoxville, and Nashville, which became prominent as a result of various railroad, industrial, and educational and cultural enterprises—all within the last two decades of the nineteenth century—indicate urban economies serving other masters besides or excluding King Cotton.

These new urban centers together with the proliferating cotton marketing and processing towns represent a shift from the antebellum pattern of one primary city—New Orleans—with a few secondary seaports and a host of small ur-



ban communities. The shift did not, however, signify a change in the nature of Southern urbanization but merely a change in geography. The South remained overwhelmingly a region of the small urban settlement.<sup>24</sup>

As long as staple crop cultivation, especially cotton, characterized Southern agriculture, this pattern remained. In the 1930s and 1940s, federal policy, mass migration, and mechanization diminished the significance of staple cultivation. In 1940, cities of less than ten thousand still typified the region's urban growth. In that year, one-third of the nation's population lived in cities of over one hundred thousand inhabitants; only one out of eight Southerners did. During the 1940s, small-city urbanization began to decline in importance. That this occurred simultaneously with the transformation of Southern agriculture indicates that staple marketing no longer sustained urban development. Industry and service activities became greater determinants of urbanization in the post-World War II South. For the first time in its history, the growth of cities of more than ten thousand inhabitants exceeded the rate for cities and towns of less than ten thousand. Two genuine metropolises now anchored the Southern urban system—Atlanta in the east and Dallas in the west. Both cities owed their early prosperity to cotton; but finance, diversified commerce, and industry built them into regional pacesetters. Finally, the most rapid urban growth in the region occurred in Florida, where agriculture had only an indirect impact on urbanization. Miami and Tampa-St. Petersburg counted almost one million persons between them in 1950. From the Florida panhandle to eastern Texas, however, the small city remained the characteristic urban settlement. Low in density, these communities continued to grow horizontally in the 1950s.<sup>25</sup> C. Vann Woodward has called this semi-urban sprawl "rurbanization," a term that captures well the hybrid form of Southern urban settlement.<sup>26</sup>

A MORE SUBTLE AND PERVASIVE RURAL QUALITY than size and concomitant marketing functions lingered as well: the rural values of the millions of migrants who, over the centuries, moved from the countryside to the city. The Wirthian concept of urbanization advanced two assumptions about urbanization and culture: the urban environment modified or destroyed migrant cultures, and cities

<sup>24</sup> On the postbellum Southern urban system through the 1920s, see Curry, "Urbanization and Urbanism in the Old South," 55-60; L. Tuffly Ellis, "The New Orleans Cotton Exchange: The Formative Years, 1871-1880," *JSH*, 19 (1973): 545-47; T. Lynn Smith, "The Emergence of Cities," in Vance and Demerath, *The Urban South*, 25-27; Kenneth Weiher, "The Cotton Industry and Southern Urbanization, 1880-1930," *Explorations in Economic History*, 14 (1977): 122-24; Clyde E. Browning and Richard Garrity, "Southern Urban Evolution: A Cartographic Portrayal," paper presented at the Conference on the Urban South, held in Norfolk, Va., February 1977; and James M. Russell, "Atlanta: Gate City of the South, 1847-1885" (Ph.D. dissertation, Princeton University, 1971), chap. 5.

<sup>25</sup> On the Southern urban system since the 1920s, see Naylor and Clotfelter, *Strategies for Change in the South*, 223; George B. Tindall, *The Emergence of the New South, 1913-1945* (Baton Rouge, 1967), 95; Reissman, "Urbanization in the South," 79-100; Smith, "The Emergence of Cities," 29-37; Jonathan Weiner, "Class Structure and Economic Development in the American South, 1865-1955," *AHR*, 84 (1979): 990; and Browning and Garrity, "Southern Urban Evolution."

<sup>26</sup> Woodward, *The Burden of Southern History* (New York, 1961), as quoted in Watters, *The South and the Nation*, 236.

functioned as disseminators of culture to the hinterland. Recent studies on ethnicity demonstrate, however, that immigrants' premigration cultures persisted in cities, despite the adverse impact of the city on immigrant life. As Kathleen Neils Conzen has pointed out, "considerable residues of ethnic culture can remain among socially assimilated individuals,"<sup>27</sup> so that the strength of these cultural values can be maintained over time. More pertinent to the Southern urban situation are studies of value persistence among native rural migrants. In 1941, anthropologist Robert Redfield discovered a broad range of values and institutions that remained intact or were only slightly modified among the rural migrants of urban Mexico; voluntary associations and kinship patterns persisted in the urban environment. More recently, African scholars have demonstrated not merely the maintenance of but the increase in tribal consciousness among transplanted rural Africans, especially in those cities where intergroup conflict is strong.<sup>28</sup> This finding may have particular relevance for the persistence of rural black folk culture amid the hostile white environment of the Southern city. Not only in the Third World but in Western societies as well, the resilience of rural values in an urban setting is a prevalent phenomenon.<sup>29</sup>

Anthropologists, in view of these findings on rural cultural persistence, have questioned the city's role as a disseminator of culture. Robert Redfield and Milton B. Singer have distinguished between primary and secondary urbanization. In the primary phase, cities carry the region's culture forward "into systematic and reflective dimensions." The city organizes and refines this core culture but does not alter it. During the secondary phase of urbanization—a phase induced through vast technological changes and the influx of diverse peoples—the cultural flow approaches the direction assumed by the Wirthian model. Even here, however, "the processes of cultural innovation . . . are far too complex to be handled by simple mechanical laws concerning the direction, rate, and 'flow' of cultural diffusion between 'city' and 'country.'" Redfield and Singer termed primary cities "orthogenetic" and secondary cities "heterogenetic."<sup>30</sup>

Southern cities have evinced orthogenetic cultural patterns for most of the past two centuries—that is, rural folkways have persisted in Southern cities. In the nineteenth century, the roots of the Southern urban population were sunk deep in rural soil, especially in the postbellum era. As early as 1868, one-half of Atlanta's population had arrived from the countryside in the three years since the end of the war. Rural migration receded in the 1870s, but a steady stream

<sup>27</sup>Conzen, "Immigrants, Immigrant Neighborhoods, and Ethnic Identity: Historical Issues," *Journal of American History*, 66 (1979): 613.

<sup>28</sup>For a summary of some of the recent work on African urbanization, most notably that in Kenneth L. Little, *West African Urbanization* (Cambridge, 1965), and H. Miner, ed., *The City in Modern Africa* (New York, 1967), see Berry and Kasarda, *Contemporary Urban Ecology*, 377.

<sup>29</sup>The question of rural cultural impact on urbanization is currently attracting considerable research interest in Sweden. For a recent effort that supports the view of rural cultural retention in an urban setting, see Sven B. Ek, *Stadens födelse* (Eslov, 1978).

<sup>30</sup>Redfield and Singer, "The Cultural Role of Cities," *Economic Development and Cultural Change*, 3 (1954): 59, 71. Southern sociologist Edgar T. Thompson has employed the concept of "orthogenetic city" to define such antebellum Southern cities as Charleston, Savannah, Mobile, and New Orleans; Thompson, "God and the Southern Plantation System," in Samuel S. Hill, Jr., ed., *Religion and the Solid South* (Nashville, Tenn., 1972), 57–91.

flowed into Southern cities for the remainder of the century. Memphis, one of the region's few prosperous larger cities, was also its most rural. By 1900, 80 percent of the city's residents were from the adjacent Mississippi or Tennessee countryside. Percentages of recent rural migrants were almost as high in Jacksonville, Birmingham, and Atlanta. While rural proportions increased, the immigrant population usually declined. In Memphis, for example, 37 percent of the population was foreign-born in 1860; by 1900, only 15 percent was.<sup>31</sup> Southern cities at the turn of the century were becoming both more rural and less diverse.

With the agricultural transformation during and after the Depression, migration from the farms quickened. As author Lillian Smith noted succinctly in the 1940s, "people have moved to town."<sup>32</sup> While the region's farm population declined by 20 percent between 1940 and 1945, Southern cities increased by nearly 30 percent, exceeding the rate of urban growth in other regions. The acceleration of rural migration and agricultural diversification continued into the next decade. In 1950, South Carolina still produced over 700,000 bales of cotton. By 1960, cotton cultivation had virtually disappeared as the white fields receded before the green wave of pasture, soybeans, and corn. The croppers and tenants—over one hundred and fifty thousand of them—who worked those cotton fields were mostly gone, to small cities like Columbia, Spartanburg, and Greenville.<sup>33</sup>

In addition to their demographic impact, the rural migrants brought their distinctive cultural baggage to the city. Although the effect of rural values on Southern urban development is not clear, several suggestions can be ventured. Family and religion were Southern rural bulwarks. "The family," Southern historian Francis Butler Simkins wrote, "was the core of Southern society; within its bounds everything worthwhile took place." The strength of family ties possibly meant the weakness of the community or collective ethic in Southern cities; kinship patterns determined social standing, and tradition counted more than novelty. Honor, vengeance, and pride, especially when women were involved, were above all family values, and they governed behavior outside the home as well. Southerners believed and practiced, as historian Gerald M. Capers has noted, "the right of private vengeance."<sup>34</sup> New South urban murder rates, for example, were typically higher than for cities in other regions. During the early 1900s, the national homicide rate per one hundred thousand inhabitants was 7.2; every Southern city over twenty-five thousand exceeded that rate. Memphis, the most rural (in terms of its population) of major Southern cities, was

<sup>31</sup> William D. Miller, *Memphis during the Progressive Era, 1900-1917* (Memphis, 1957), 7-9, and "Myth and New South Murder Rates," *Mississippi Quarterly*, 26 (1973): 146; and Eugene J. Watts, *The Social Bases of City Politics: Atlanta, 1865-1903* (Westport, Conn., 1978), 90.

<sup>32</sup> Smith, *Killers of the Dream* (New York, 1949), 41.

<sup>33</sup> Ernest M. Lander, Jr., "Introduction," in Lander and Richard J. Calhoun, eds., *Two Decades of Change: The South since the Supreme Court Desegregation Decision* (Columbia, S.C., 1975), 2-3.

<sup>34</sup> Simkins, *A History of the South* (3d edn., New York, 1968), 388, as quoted in George B. Tindall, "Beyond the Mainstream: The Ethnic Southerners," *JSH*, 40 (1974): 15; and Capers, "The Rural Lag on Southern Cities," *Mississippi Quarterly*, 20 (1967): 261. Also see Smith, *Killers of the Dream*, 141.

also the nation's murder capital, with a rate of 47.1; Charleston was a distant second at 27.7.<sup>35</sup>

The violence contrasted with, yet was curiously connected to, the deep religiosity of the rural migrants. The Southern church, historian George B. Tindall wrote, "is something unique in all Christendom in its single-minded focus on salvation, its sense of assurance, and its rejection . . . of other versions of Christian experience. It serves as one of the chief instruments of ethnic solidarity."<sup>36</sup> As Tindall's description implies, it was not the deep religiosity of Southerners that was unique but, rather, the philosophical structure of their religious devotion. Evangelical Protestantism, which first swept over the frontier South in the 1830s, came increasingly after the Civil War to define Southern religious practices and principles and to mark religion as one of the distinctive features of Southern identity.

Specifically, this Southern religious tradition was pessimistic, emphasizing man's basic depravity, with the opportunity of individual salvation only through a conversion experience. The evangelical sects (primarily Baptists, Methodists, and Presbyterians) and their ministers were well established by 1860, but not until after the Civil War, when Southern religion became entwined with the Lost Cause, did evangelical Protestantism pervade every cultural pore of the region. The pain and the martyrdom evoked by the Lost Cause blended well with a religion obsessed with suffering and salvation. For a region wrapped in grief and defeat, where dull poverty was commonplace, evangelical ritual, liturgy, and promise filled a deep spiritual need. By 1900, a distinctive civil religion was evident, distinguishing Southern culture from the optimistic, scientific, social gospel sweeping Northern ecclesiastics and society. What began as a fervent rural pastime in the early nineteenth century became a regional hallmark by the beginning of the twentieth.<sup>37</sup>

Evangelical Protestantism affected Southern culture by standing as a regional bulwark against change. The focus on individual sin and salvation and on otherworldly rewards regardless of earthly deprivations shifted attention from society to the individual and his compact with God. "Christ-centeredness," Southern theologian Samuel S. Hill, Jr., averred, "easily shades off into fearful self-centeredness." Individual suffering, poverty, and pain "have to be endured"

<sup>35</sup> Miller, "Myth and New South Murder Rates," 143-50. John Shelton Reed discovered the persistence of such rural regional characteristics as localism, violence, and religiosity in Southern cities into the 1960s. The persistence and pervasiveness of these factors, Reed argued, set the region apart from the rest of the nation. See Reed, *The Enduring South* (rev. edn., Chapel Hill, 1975), esp. 89-90.

<sup>36</sup> Tindall, "Beyond the Mainstream: The Ethnic Southerners," 16.

<sup>37</sup> For the rise of evangelical Protestantism in the antebellum era, see Anne C. Loveland, *Southern Evangelicals and the Social Order, 1800-1860* (Baton Rouge, 1980); and, for the late nineteenth and twentieth centuries, see Kenneth K. Bailey, *Southern White Protestantism in the Twentieth Century* (New York, 1964); Hill, "The South's Two Cultures," in Hill, *Religion and the Solid South*, 39; Anne Firor Scott, "Women, Religion, and Social Change in the South, 1830-1930," *ibid.*, 92-116; and Charles Reagan Wilson, "The Religion of the Lost Cause: Ritual and Organization of the Southern Civil Religion, 1865-1920," *JSH*, 46 (1980): 219-38. These authors concur that sectional denominationalism and the philosophical bases of evangelical Protestantism pervaded Southern identity by the late nineteenth century and set the region apart from the rest of the country. Even today, as Bailey has pointed out, "perhaps in the sphere of religion . . . Southern identity is best delineated"; *Southern White Protestantism in the Twentieth Century*, ix.

since "the Christian is merely a sojourner in this world" and must await reward in the next.<sup>38</sup> These precepts readily shunted attention from the very real-world injustice of a biracial society, for example. The great upsurge in revivalism between 1890 and 1906 coincided with the nadir of black fortunes in the South. Finally, whatever ministerial interest in social reform prevailed was diffused by the emphasis upon individual salvation and, especially in an urban setting, upon the number of individuals brought into the flock. "Being a Christian," Southern theologian Langdon Gilkey observed, "thus becomes merely the operation of expanding itself."<sup>39</sup>

Evangelical Protestantism was and is replete with certainties: the certainty of sin, the certainty of salvation through conversion, and the infallibility of the Bible. Since change means uncertainty, to allow any change would undermine faith and, eventually, Southern society. Indeed, Southern religion was virtually indistinguishable from Southern society in the late nineteenth century. As Hill observed, "Southern mores are accorded a certain divine quality. They are not only the way things are, they are the way things should be." Novelist Thomas Wolfe stated it in more secular terms when he decried the "hostile and murderous entrenchment against all new life" that permeated the region in the 1920s.<sup>40</sup>

As a part—perhaps the most dominant part—of Southern culture, evangelical Protestantism invariably affected Southern urbanization, and vice versa. Although the evangelical sects first prospered on the Southern frontier, the spirit of revivalism soon penetrated the cities and towns of the antebellum South. The antebellum evangelists appealed to the "middling ranks of Southern society" and thus found numerous adherents in the urban South. Then, too, the evangelists' efforts to reach the greatest numbers for conversion and salvation meant that cities became logical centers for evangelical activity. The sheer size of the urban populations and the frequent opportunities to speak outside the church afforded the evangelist a "wider influence than he could ever hope for in the country."<sup>41</sup>

After the Civil War, religion as a Southern cultural mainstay flourished in Southern cities and became inseparable from urban life. "Church was our town," author Lillian Smith recalled of her childhood during the early decades of the twentieth century. Wood planks or carpeting may have replaced the dirt floor of the forest, but the old enthusiasm and demonstrative preaching of guilt, sin, fear, and salvation persisted in the urban milieu. The evangelical church's "general spirit and outlook were transplanted to the nearby town center," Edgar T. Thompson reminisced. "Southern town and even city churches generally might almost be described as transplanted rural institutions," he concluded.<sup>42</sup>

<sup>38</sup> Hill, "Toward a Charter for a Southern Theology," in Hill, *Religion and the Solid South*, 192, and "The South's Two Cultures," *ibid.*, 41.

<sup>39</sup> Gilkey, *How the Church Can Minister to the World without Losing Itself* (New York, 1929), 64n, as quoted in Hill, "Toward a Charter for a Southern Theology," 198.

<sup>40</sup> Hill, "The South's Two Cultures," 45; and Wolfe, *Look Homeward, Angel* (New York, 1929), 155, as quoted in C. Hugh Holman, "The Dark, Ruined Helen of His Blood: Thomas Wolfe and the South," in Leslie A. Field, ed., *Thomas Wolfe: Three Decades of Criticism* (New York, 1968), 17.

<sup>41</sup> Loveland, *Southern Evangelicals and the Social Order*, 32, 50.

<sup>42</sup> Smith, *Killers of the Dream*, 85; and Thompson, "God and the Southern Plantation System," 57–58.



Church and urban society remain closely connected. Religious affiliation is as important as lineage in describing a person today. The Southern urban church is the best place in the city to attain and maintain social and business contacts. And, even though framed by expensive architecture, the basic principles of evangelical Protestantism continue to issue forth from urban pulpits, especially in the numerous Baptist and Christian congregations.<sup>43</sup>

Evangelical Protestantism and the values associated with it have maintained the orthogenetic character of Southern cities perhaps more than staple agriculture has. By blocking out ideological competitors and by supporting traditional beliefs, Southern urban religion helped make Southern cities bastions of conservatism, if not reaction, rather than of change, as in cities elsewhere in the United States. Evangelical churchmen, concerned with individual salvation or fearful of disrupting their flocks, generally either ignored or supported the biracial society—slavery in the antebellum period and segregation after the Civil War. Even so-called liberal churchmen in the twentieth century “condemned forces which they felt encouraged religious and social diversity within the city.”<sup>44</sup> Since evangelical precepts also divided society into absolutes of good and evil, laymen rarely questioned the status quo of urban society that the church had sanctified as the holy order. “Our first lesson about God made the deepest impression on us,” Lillian Smith related. “We were told that He loved us, and then we were told that He would burn us in everlasting flames of hell if we displeased Him.” Naturally, every child hoped to avoid that fate. “The best way,” Smith concluded, “was never to question anything but always accept what you were told.”<sup>45</sup>

The evangelical faiths facilitated the perpetuation of myths—about slavery, the Lost Cause, and the New South. The role of the church complemented that of the ubiquitous booster who, in his exaggerated rhetoric, revealed the same defensiveness, insecurity, and self-righteousness that the evangelical ecclesiastics exhibited.<sup>46</sup> The “commercial-civic elite,” as Blaine A. Brownell called such boosters, flourished in both the Old and the New South, but rarely as virulently as in the decades after World War I; their rhetoric adopted religious metaphors (indeed, Paul M. Gaston’s New South “Creed” implies a religious connotation), and they proclaimed the ecumenicism of their policies, generally opposing change.<sup>47</sup> In the early twentieth century, for example, urban leaders converted new methodologies like planning to conservative objectives designed to preserve the existing social and political structure.<sup>48</sup> They dissembled to avoid pursuing

<sup>43</sup> See Erskine Caldwell, *In the Shadow of the Steeple* (London, 1967), 84–85, 129–34; and Bailey, *Southern White Protestantism in the Twentieth Century*, 152–54.

<sup>44</sup> Wayne Flynt, “Religion in the Urban South: The Divided Religious Mind of Birmingham, 1900–1930,” *Alabama Review*, 30 (1977): 125.

<sup>45</sup> Smith, *Killers of the Dream*, 85, 86, 99.

<sup>46</sup> See Hill, “The South’s Two Cultures,” 50.

<sup>47</sup> See Brownell, *The Urban Ethos in the South*, 47–48; and Gaston, *The New South Creed: A Study in Southern Mythmaking* (New York, 1970).

<sup>48</sup> See Howard L. Preston, *Automobile Age Atlanta: The Making of a Southern Metropolis, 1900–1935* (Athens, Ga., 1979), 102; Blaine A. Brownell, “The Commercial-Civic Elite and City Planning in Atlanta, Memphis, and New Orleans in the 1920s,” *JSH*, 41 (1975): 339–68; and Christopher Silver, “Urban Planning and Urban Development in the New South: Richmond, 1900–1960,” paper presented at the Conference on the New South, held in Norfolk, Va., February 1978.

policies that could threaten their hegemony. When the Memphis Chamber of Commerce boasted in early 1941 that "there is no housing shortage in Memphis. . . . Memphis is well-housed," the city had one of the worst housing shortages in the nation, and nearly four out of every five blacks and one out of three whites were living in substandard housing. Above all, like the ministers, they claimed to be the keepers of the community's welfare, creating an identity between community welfare and the leaders' policies. In this manner, as Richard Sennett has noted, "the image of community is purified of all that might convey a feeling of difference, let alone conflict, on who 'we' are."<sup>49</sup> This was precisely the thrust of evangelical Protestantism, of course: purification, antipathy to change, and a sharp distinction between "we" and "them."

Although boosters and ministers occasionally clashed in the early decades of the twentieth century on, for instance, blue laws, their objectives frequently coincided. It was not unusual to see Bishop Warren A. Candler and the *Manufacturers' Record* agreeing that the Scopes trial was "one of the South's supremest advertisements." The *Record* also noted with approval that the "agnosticism and atheism so prevalent throughout the North and West" was relatively unknown in the South. In smaller urban settlements, like Lubbock, Texas, the interface between preacher and politician was, perhaps, more complete: "They [the ranchers and merchants] and the preachers joined in the enterprise of creating an orderly community safe for churches and secure for business enterprises."<sup>50</sup> As the urban boosters borrowed freely from the church preachings, the ministers, especially after the 1930s, utilized booster promotion tactics in revivals and membership campaigns. Revivals were a most successful business enterprise that required sophisticated organizing and advertising strategies. Although Atlanta novelist Ward Greene despaired of "howling god-hoppers running the town for purity and pep" during the 1930s, revivals were one of the few exciting and lucrative activities in the Depression-ridden South.<sup>51</sup> After World War II, when membership drives occupied an increasing amount of ecclesiastic attention, booster tactics proved especially availing. The *Georgia Christian Index* observed with approval that "many tricks of the advertising trade can be adapted to the promotion of the church and its program."<sup>52</sup>

The close attention urban clerics and boosters paid to citizens' civic and spiritual ideals seems to have exhausted their communitarian spirit. Churchmen and

<sup>49</sup> Memphis Chamber of Commerce, *A Brief Survey of Industrial Opportunities* (rev. edn., Memphis, 1941), as quoted in Robert A. Sigafos, *Cotton Row to Beale Street: A Business History of Memphis* (Memphis, 1979), 208; and Sennett, *The Uses of Disorder: Personal Identity and City Life* (New York, 1970), 33, as quoted in Brownell, *The Urban Ethos in the South*, 219.

<sup>50</sup> Candler, "Liberalism Proposing to Liberate the South," Nashville, Tennessee, *Christian Advocate*, September 18, 1925, as quoted in Bailey, *Southern White Protestantism in the Twentieth Century*, 91; and Merton L. Dillon, "Religion in Lubbock," in Lawrence L. Graves, ed., *A History of Lubbock*, pt. 3 (Lubbock, 1961): 457, as quoted in Hill, "The South's Two Cultures," 30.

<sup>51</sup> Greene, *Ride the Nightmare* (New York, 1930), 72, as quoted in Blaine A. Brownell, "The Urban South Comes of Age," in Brownell and Goldfield, *The City in Southern History*, 144. Of course, revivalists engaged in promotional activities at least a century earlier, and it may be argued that the economic snake oil peddled by Southern urban boosters owed its success to minds already trained and "organized" by effective preachers of the gospel.

<sup>52</sup> Macon, Georgia, *Christian Index*, December 10, 1942, as quoted in Bailey, *Southern White Protestantism in the Twentieth Century*, 132.

businessmen generally neglected the social needs of their constituents. With a religious philosophy that emphasized individual sin and salvation and a booster creed that stressed public unity for economic development and racial stability, social policies were superfluous, diversionary, and potentially disruptive.<sup>53</sup> Not surprisingly, from the mid-nineteenth century to the present day, per capita expenditures in Southern cities, especially for social services, have lagged far behind similar expenditures in other cities. In 1902, for example, at a time of high spiritual and civic revival, no Southern city spent the national per capita average for education (\$4.37), and most educational spending was one-third to one-half less than that average. From libraries to street paving to public health services, Southern cities traditionally lagged behind cities elsewhere. As late as 1970, no Southern city matched or exceeded the national average per capita urban expenditure for public welfare (\$11.98).<sup>54</sup>

The urban South's dismal performance on social services restricted urban development. Nobel laureate economist Theodore W. Schultz devised the concept of human capital as an essential programming element for developing areas.<sup>55</sup> "The decisive factor," Schultz declared, "is the improvement in population quality."<sup>56</sup> Education, health, and housing of the poor are the most important improvements and investments in human capital. The paucity of Southern investments in these areas has reduced the quality of the regional population and, hence, its ability to contribute to the region's development.

THE AVERSION OF THE CIVIC AND RELIGIOUS ELITE to such investments in human capital resulted not only from the philosophical bases of booster and evangelical ideology but also from the immense presence of a biracial society. Too much improvement in population quality implied the threat of population equality—a situation that would have eroded "civic-religious" hegemony and, therefore, the foundation of regional society. Accordingly, the region's economic and religious institutions sought to regulate, separate, isolate, and subjugate the black race. Since urban life created greater opportunities for racial interaction, urban whites were as vigilant as, if not more vigilant than, their rural counterparts in maintaining the biracial society.<sup>57</sup>

<sup>53</sup> On the poor social record of the Southern "civic-religious" leadership, especially in the twentieth century, see Caldwell, *In the Shadow of the Steeple*, 84–85; Brownell, "The Urban South Comes of Age," 153; and Hill, "The South's Two Cultures," 36, 41, 48–49. On the nineteenth century, see Loveland, *Southern Evangelicals and the Social Order*, 119, 161–62; and Scott, "Women, Religion, and Social Change in the South, 1830–1930," 116.

<sup>54</sup> On inadequate social services in the antebellum period, see Goldfield, "Pursuing the American Urban Dream: Cities in the Old South," 68–83; in the late nineteenth century, see Howard N. Rabinowitz, "Continuity and Change: Southern Urban Development, 1860–1900," in Brownell and Goldfield, *The City in Southern History*, 110–12; and, for the contemporary Southern city, see Peter A. Lupsha and William J. Siembieda, "The Poverty of Public Services in the Land of Plenty: An Analogy and Interpretation," in David C. Perry and Alfred J. Watkins, eds., *The Rise of the Sunbelt Cities* (Beverly Hills, 1977), 173.

<sup>55</sup> The clearest statement of this concept appears in Theodore W. Schultz, "The Economics of Being Poor," Nobel Lecture, December 8, 1979, Stockholm, Sweden.

<sup>56</sup> Schultz, "The Economics of Being Poor," 2. Demographic shifts to the Sunbelt have not upset the regional value structure but have tended, instead, to reinforce it. The predominantly suburban and white-collar migrants have sought low taxes and racial homogeneity as much as employment opportunities and a temperate climate.

<sup>57</sup> See Brownell, "The Urban South Comes of Age," 146; and Rabinowitz, "Continuity and Change: Southern Urban Development," 122.

The biracial society in an urban setting restricted blacks to certain low-level occupations. Agriculture and its institutions—slavery in the antebellum era and sharecropping, tenancy, crop lien, and peonage following the Civil War—restricted the free flow of black labor to the cities. This restriction provides yet another example of how agriculture controlled the economic life of the city. Migrating to the city, the blacks' primarily agrarian skills were ill suited to urban jobs and ensured black employment only in low-status occupations.<sup>58</sup> The biracial system almost guaranteed that blacks stayed there. Although cities such as Charleston and New Orleans had a small black elite in the antebellum period, the black's occupational status suffered drastically with emancipation and increased migration to the cities.<sup>59</sup> The existence of such a relatively large body of marginal consumers further lowered demand and capital accumulation in the urban South and provided an economic excuse for the poor, selective quality of public services.

The Civil War also produced changes in the blacks' residential status in ways that solidified the biracial society. The change from exclusion to segregation, which Howard N. Rabinowitz has advanced as the regional biracial pattern after 1865,<sup>60</sup> allowed blacks access to some features of urban life that they had not enjoyed prior to the Civil War but did not, however, signify an urban departure from traditional regional racial mores. As orthogenetic entities, Southern cities "carried forward," rather than transformed, regional racial patterns. Segregation was such an adjustment, much as crop lien and peonage were racial accommodations in the rural districts. Segregation may have allowed blacks greater participation, but they could only participate on white terms. Moreover, segregation generalized blacks, ruthlessly isolating them from the mainstream of urban life; and residential patterns show most clearly the impact of segregation.

Black residential patterns in Southern cities were distinctive, resembling more the spatial characteristics of lower-caste groups in developing societies than the neighborhood arrangements of Northern cities. Some residential dispersion existed in the antebellum era due to the necessities of slavery. Rather than the single large ghetto found in Northern cities, concentrations of blacks that reflected the scattered nature of undesirable housing sites emerged in the post-bellum South. In 1877, the Nashville Board of Health reported that the city's blacks "reside mainly in old stables, situated upon alleys in the midst of privy vaults, or in wooden shanties a remnant of war times, or in huts closely crowded together on the outskirts."<sup>61</sup> These "neighborhoods," especially those on the pe-

<sup>58</sup> On the limits that agricultural labor systems placed upon both blacks and urbanization, see Claudia D. Goldin, *Urban Slavery in the American South, 1820-1860: A Quantitative History* (Chicago, 1976); Jay R. Mandle, *The Roots of Black Poverty: The Southern Plantation Economy after the Civil War* (Durham, N.C., 1978); Roger L. Ransom and Richard Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation* (Cambridge, 1977); Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978); and Ralph V. Anderson and Robert E. Gallman, "Slaves as Fixed Capital: Slave Labor and Southern Economic Development," *JSH*, 44 (1977): 24-46.

<sup>59</sup> For a superb account of blacks and Southern cities, see Howard N. Rabinowitz, *Race Relations in the Urban South, 1865-1890* (New York, 1978).

<sup>60</sup> Rabinowitz, *Race Relations in the Urban South*, esp. 125-254.

<sup>61</sup> Nashville, *Report of the Board of Health, 1876-1877* (Nashville, Tenn., 1877), 108, as quoted in Rabinowitz, *Race Relations in the Urban South*, 118.

riphery, presented a primitive, rural appearance: the dirt roads, outdoor facilities, poor drainage, and frame "double-pen" houses or "shotgun shacks" differed little from sharecroppers' dwellings. And, by the 1890s, the lines dividing black and white residential communities had hardened.<sup>62</sup>

Housing clusters continued to characterize black residential patterns in the urban South during the twentieth century—typically, one large cluster in the most decrepit area near the center, surrounded by smaller clusters moving outward toward the periphery. The peripheral neighborhoods were frequently either the "temporary" communities that sprouted to house freedmen immediately following the Civil War or erstwhile rural areas annexed by the Southern cities' voracious appetite for land. In some of the older cities like Savannah and Charleston, reminders of the antebellum past persisted in the 1920s and 1930s, as long fingers of blacks' residences intruded into white neighborhoods on the narrow lanes behind the major residential thoroughfares. But these vestiges disappeared after World War II as whites began to abandon the centers, and residential patterns similar to those in newer Southern cities replaced such antebellum holdovers. Birmingham, for example, had a primary black neighborhood adjacent to the downtown area and several smaller clusters scattered wherever poor urban land existed.<sup>63</sup>

The black isolation from white areas has increased during this century. By 1960, only 5.5 percent of the Southern urban population resided in integrated neighborhoods compared with 31.8 percent in the Northeast. Although black neighborhoods in the South were more dispersed than they were in Northern cities, they were usually more segregated. In addition, Southern urban blacks are still more likely to be peripheral and suburban residents than their Northern counterparts are, a legacy from Reconstruction and annexation. In 1970, 14 percent of the South's metropolitan black population resided on the metropolitan periphery, compared with only 3 percent in the Northeast.<sup>64</sup>

Black ecological patterns in Southern cities in the last one hundred years have resembled those found in pre-industrial societies of the Third World. Peripheral settlement in poor housing is a common spatial phenomenon. Analysis of social areas—that is, census tracts and neighborhoods—in cities indicates that Southern cities conform to non-Western patterns primarily because of the presence and the character of the black population: a separate and unequal society within the region and its cities. The virtual identity between ethnicity and socioeconomic status—a characteristic of social areas in underdeveloped so-

<sup>62</sup> On black residential patterns during the late nineteenth and early twentieth centuries, see John Kellogg, "Negro Urban Clusters in the Postbellum South," *Geographical Review*, 68 (1978): 311–21; and Zane L. Miller, "Urban Blacks in the South, 1865–1920: The Richmond, Savannah, New Orleans, Louisville, and Birmingham Experience," in Leo F. Schnore, ed., *The New Urban History: Quantitative Explorations by American Historians* (Princeton, 1975), 199–200.

<sup>63</sup> On black residential patterns since the 1920s, see Brownell, *The Urban Ethos in the South*, 26–28; Ronald H. Bayor, "Ethnic Residential Patterns in Atlanta, 1880–1940," *Georgia Historical Quarterly*, 44 (1979): 435–47; Nicholas J. Demerath and Harlan W. Gilmore, "The Ecology of Southern Cities," in Vance and Demerath, *The Urban South*, 135–64; and Dana F. White and Timothy J. Crimmins, "Urban Structure, Atlanta," *Journal of Urban History*, 2 (1976): 231–52.

<sup>64</sup> Carl Abbott, "Colonial Place, Norfolk: Residential Integration in a Southern Urban Neighborhood," typescript kindly provided by the author.



cieties—describes the separate black communities in Southern cities.<sup>65</sup> Sociologist Allison Davis's description of this convergence in 1941 could be applied to Indian or Middle Eastern outcast groups:

Life in the communities in the Deep South follows an orderly pattern. The inhabitants live in a social world clearly divided into two ranks, the white caste and the Negro caste. These colored castes share disproportionately in the privileges and obligations of labor, school, and government.<sup>66</sup>

Regional biracialism has given the Southern city a non-Western character, perhaps more than any other regional characteristic has.

In 1941, researchers undoubtedly believed that biracialism had created a unique urban social environment that would persist as long as color remained a decisive factor in regional life. But biracialism possessed within it, ironically, the potential for regional and urban change. The blacks themselves, first alone and then with outside support, produced alterations in Southern urban society, especially following World War II. The vestiges of biracialism, however, remain, and race continues to play its distinctive role in the region and its cities. Black political power, for example, is more shadow than substance. Blacks have secured only a minimum of white political support. In 1979, when Birmingham elected its first black mayor, Richard Arrington, only 10 percent of the white voters supported him. Black political strength and security depend on sheer numbers, not on changing attitudes in the white community. Several Southern cities have already attempted to push through reapportionment, at-large elections, annexation, and consolidation schemes under the guise of political reform, when actually these policies have been aimed at diluting black political strength.<sup>67</sup>

The stakes of urban political power, however, are becoming less important. When blacks finally ascend to the highest offices in urban politics, they soon discover that they have inherited, as historian James C. Cobb has noted, "more of a problem than a prize."<sup>68</sup> As the pace of annexation slows and as white residents and commerce continue to abandon the Southern city, economic problems mount. Black political power is circumscribed by the absence of black economic power.

THE BLACKS' ECONOMIC DEFICIENCY is shared, in a relative sense, by the region and its cities. Rural agricultural patterns and values (especially evangelical

<sup>65</sup> For a discussion of Third World urban ecological patterns, see Berry and Kasarda, *Contemporary Urban Ecology*, 154–55.

<sup>66</sup> Davis *et al.*, *Deep South: A Social Anthropological Study of Caste and Class* (Chicago, 1941), 539, as quoted in Berry and Kasarda, *Contemporary Urban Ecology*, 155.

<sup>67</sup> For discussions concerning the limits of black political power in the urban South, see Numan V. Bartley, "Atlanta Elections and Georgia Political Trends," *New South*, 25 (1970): 22–30; Cobb, "Urbanization and the Changing South," 253–66; Samuel Dubois Cook, "Southern Politics since 1954: A Note on Change and Continuity," in Lander and Calhoun, *Two Decades of Change*, 5–19; and Virginia H. Hein, "The Image of 'A City too Busy to Hate': Atlanta in the 1960s," *Phylon*, 33 (1972): 205–21.

<sup>68</sup> Cobb, "Urbanization and the Changing South," 262.

Protestantism) and biracialism have limited capital accumulation and have skewed investment patterns to labor and land, rather than to specifically city-building enterprises. A persistent colonialism has reinforced these indigenous factors—ruralism and biracialism—to form the third major element in the region's eternal triangle.

Colonialism denotes a colonial power. In the eighteenth century, it was the British, but, in the nineteenth and twentieth centuries, it has been—and is—the North. The development of a national economy, centered in New York beginning in the 1840s, fastened a type of regional specialization upon the South that remains with it to the present day. The South as the producer and occasionally the basic processor of raw materials has been in continuous economic servitude to the North not only for manufactured products but also for all of the financial, credit, legal, accounting, and factoring services that attend a national economy. Cities are the "instruments" of regional articulation in a national economy and set the economic tone for the region. Thus, cities were regional colonial headquarters as the South sank deeper into economic dependence. Southern cities served as collection points for and funnels to Northern centers and as distribution points for the return flow. This system limited capital accumulation in the region, which reduced the opportunities for the region to develop beyond its colonial economy. And everything limited urbanization. In fact, as the national economy grew after the Civil War, Southern cities shrunk relative to their Northern (national) counterparts.<sup>69</sup>

Northern investors encouraged the economic development of the region only insofar as it enhanced their dominance in the national economy. After the Civil War, Northern control of Southern railroads, for example, ensured profitable and speedy transfers to and from the colonial region. In the antebellum era, however, local investors dominated Southern railroad directorships. The Panic of 1873 dealt struggling Southern-owned companies a financial death blow, and Northern capital willingly revived the corpse. In 1870, Northerners comprised only 19 percent of the South's railroad directors; by 1880, that percentage had reached 37. By 1900, when financial capitalists like J. P. Morgan consolidated and re-organized the nation's railroad systems, Northern financiers controlled the South's five major rail lines, and 60 percent of the directors were Northerners.<sup>70</sup>

The patterns of Northern investment in the South were selective, but, even so, they restricted Southern industrial development and, hence, urbanization. The potential of Birmingham's steel production frightened, rather than attracted, Northern investors in the late 1870s. Whatever advances the city made during the next decade occurred because of local capital. Eventually, during the early years of the twentieth century, the Pittsburgh Plus system and financial control

<sup>69</sup> For discussion of the formation of the national economy and its impact upon the antebellum Southern city and region, see David R. Goldfield, *Urban Growth in the Age of Sectionalism: Virginia, 1847-1861* (Baton Rouge, 1977), 235-46; Allan R. Pred, *The Spatial Dynamics of U.S. Urban-Industrial Growth, 1800-1914* (Cambridge, Mass., 1966); and Louis B. Schmidt, "Internal Commerce and the Development of the National Economy before 1860," *Journal of Political Economy*, 42 (1939): 798-822.

<sup>70</sup> Rabinowitz, "Continuity and Change: Southern Urban Development," 102, 105-06.

by U.S. Steel effectively limited Birmingham competition and assured Northern industrial superiority. Textile milling became a Northern investment beneficiary in the twentieth century. But, as an industrial activity that occurred in a predominantly rural setting, it generated relatively little capital for the region or its cities. Low wages coincided with low investments in human capital. The textile mill was an industrial plantation with all of the same human debilities as its agricultural counterpart.<sup>71</sup>

The Great Depression revealed the shallowness of the South's economy. The region's cities were the nation's "basket cases" during the 1930s. The Depression also resulted in a modification of the national economy as the federal government became a major force for economic redistribution. Federal grants accelerated the regionalization of the national economy. Southern cities were the direct beneficiaries of this process as the federal government paid for capital facilities that Northern cities had bought for themselves in earlier decades and on which they were still paying off the debt. The almost-free modernization that Southern cities received proved to be an important economic advantage in subsequent decades.<sup>72</sup>

During World War II, the redistribution of national economic advantages continued as military bases and, more significantly, industry both demonstrated the increased federal presence. Indeed, federal assistance to Southern urban industry effectively primed the pump of the regional and urban economies. The government helped stimulate new industries as, for example, in Houston and released old ones from colonial restrictions as in Birmingham. Military spending in Southern cities encouraged the development of electronics research and manufacturing firms, scientific equipment companies, and aeronautics machinery plants. The growth of high-technology industry has had a positive impact on a region traditionally burdened with low-technology, low-wage industries. Between 1940 and 1960, the high-wage industrial sector increased by 180 percent in the South (the national rate was 92 percent for the same two decades), and the industrial labor force in the region grew by more than one and a half million, with almost 90 percent of these jobs in high-wage industries. Moreover, these industries were primarily urban-based. By 1960, therefore, low-wage industries accounted for only two out of every five manufacturing jobs in the region.<sup>73</sup>

The multiplier effect of federal economic policy did not ensure regional deliverance from the inequities and unbalanced development of the old ante- and postbellum industrial regime. Just as vestiges of the biracial society cling like Spanish moss to the urban South, traditional industrial patterns based on a rural, colonial economy linger on. The textile mill culture persists. In South Caro-

<sup>71</sup> On the patterns of late-nineteenth-century Northern investment in the South, see Miller, *Memphis during the Progressive Era*, 43; Jonathan Weiner, *Social Origins of the New South: Alabama, 1860-1885* (Baton Rouge, 1978), 162-84; and Weiher, "The Cotton Industry and Southern Urbanization," 124.

<sup>72</sup> See Tindall, *The Emergence of the New South*, 476-77.

<sup>73</sup> See Lorin A. Thompson, "Urbanization, Occupational Shift, and Economic Progress," in Vance and Demerath, *The Urban South*, 38-53; and Alfred J. Watkins and David C. Perry, "Regional Change and the Impact of Uneven Urban Development," in Perry and Watkins, *The Rise of the Sunbelt Cities*, 19-54.

lina, West German flags fly in Spartanburg, Kuwaiti money flows on Kiawah, and boosters prattle innocently, yet revealingly, saying, "What we've done here ought to be done in the underdeveloped countries. We established a good political atmosphere and showed we had a real commitment to economic growth." And, indeed, the industrialization that came in a wave during the 1960s and 1970s has transformed South Carolina from a poor agricultural state to a poor industrial state. The state ranks forty-sixth in per capita income. The average mill worker earns 20 percent less per hour than the national average. South Carolina continues to lead the nation in illiteracy and infant mortality. Manufacturing remains primarily a nonmetropolitan—that is, small-town—activity.<sup>74</sup>

Colonialism persists in other areas of the Southern economy as well. While the growth of air and truck transport after World War II reduced the railroad's dominance over Southern cities, other aspects of the national economy have a distinctly northern flow. The region's cities depend upon Northern banks to finance large-scale operations. Most of Houston's corporations list a New York institution as their principal bank. Any firm in the urban South with international business connections invariably goes through New York banks. Corporations in Southern cities patronize New York and Washington law offices. They also use New York accounting firms. All of this means that Northern interests continue to control large-scale investment in Southern cities.<sup>75</sup> With major banking, accounting, and legal services for the South still based in the North, capital accumulation remains a regional problem. Economist Charles F. Haywood asserted in 1978 that "the South has long been a region of capital shortage. It remains so today and will be so for some years to come." Haywood suggested that the connection of the major Southern urban corporations with the national banking network need not impede their growth. But those activities "that are heavily dependent on local sources of funds—housing, local businesses, and . . . local government"—may experience some difficulty.<sup>76</sup> If this prediction is accurate, the South's traditional pattern of low services, small investments in human capital, and limited urban growth will continue.

The comparison advanced above between South Carolina and underdeveloped countries is apt because the South's subsidiary role in the national economy also corresponds in some degree to the role of underdeveloped nations in the world economy. Historian John H. Coatsworth, for example, located two major obstacles to economic development in nineteenth-century Mexico: "inadequate transport and inefficient economic organization." The South has been afflicted by both, and its cities have been similarly victimized. An over-reliance on rivers and the late development of a railroad system retarded regional economic development. The unorganized nature of the Southern economy based on the individual decisions of staple entrepreneurs, the restrictions on the mobil-

<sup>74</sup> *Washington Post*, April 30, 1978. Also see Dale Newman, "Work and Community Life in a Southern Textile Town," *Labor History*, 38 (1978): 204-05.

<sup>75</sup> Robert B. Cohen, "Multinational Corporations, International Finance, and the Sunbelt," in Perry and Watkins, *The Rise of Sunbelt Cities*, 211-26.

<sup>76</sup> Haywood, "The South's Future Capital Needs," *Southern Living*, January 1978, pp. 30-32.

ity of capital and labor, and the low priority of innovation or of any new ideas contributed to what Coatsworth termed for Mexico "inefficient economic organization."<sup>77</sup> Thus, the rural value system helped bring about the North's colonial control of the South. As historian David Bertelson has maintained, "The South was an individualistic, chaotic economy in an America whose other inhabitants held some idea of community purpose."<sup>78</sup>

Mexico ultimately extricated itself from its backwardness by capitalizing on its own economic vulnerability, which provided "vast comparative advantages for foreign technology and resources." In the South, especially after the Civil War, Northern capital and expertise produced a transportation system and organized the production and processing systems to suit Northern commercial and industrial requirements. This relieved Southern backwardness to some degree—but not regional dependence. Coatsworth concluded that, although Mexico was not likely to create its own viable, self-sufficient economy, the decision to turn to foreign entrepreneurs and capital produced "a long-term dependence on foreign technology, resources, and markets."<sup>79</sup> By 1865, it was obviously too late for the South to secure regional economic independence or even parity; the result was "long-term" dependence on the North. The role of Southern cities and the limits this economic system placed on their development reflected the regional situation of economic subservience.<sup>80</sup>

Southerners were aware of their economic inferiority and wrestled with its consequences for over a century. Antebellum Southerners like Virginian George Fitzhugh and New Orleans journalist J. D. B. De Bow sought to remove from the South its "indelible brand of degradation" by developing the region's cities, industries, and commercial facilities and connections.<sup>81</sup> The Civil War dashed dreams of economic equality, and urban-based boosters like Henry W. Grady and Richard H. Edmonds were more accommodationist in their approach to colonialism. Their extravagant claims and grandiose programs produced few results, but their rhetoric enabled local leaders to wrap themselves in the mantle of Southern patriotism and maintain a biracial society to secure elusive and illusory economic objectives.

It was cruel irony that the urban boosters' very striving for economic success only deepened their region's economic inferiority. They auctioned their labor to the lowest industrial bidder, sacrificed services to maintain tax advantages for prospective investors, and allowed polluters and exploiters to carve up and de-

<sup>77</sup> Coatsworth, "Obstacles to Economic Growth in Nineteenth-Century Mexico," *AHR*, 83 (1978): 91, 92.

<sup>78</sup> For this statement of Bertelson's views in *The Lazy South* (New York, 1967), 82–83, see Smiley, "The Quest for the Central Theme in Southern History," 317.

<sup>79</sup> Coatsworth, "Obstacles to Economic Growth in Nineteenth-Century Mexico," 100.

<sup>80</sup> The interaction between colonialism and urbanization in the South is evident from the inability of geographers successfully to apply central place theory to the Southern urban system, which evinces a colonial, rather than a modern, industrial pattern; see Browning and Garrity, "Southern Urban Evolution"; and Berry and Kasarda, *Contemporary Urban Ecology*, 391. On the confusion resulting from the application of ahistorical geographic models to the Southern urban system, also see Rudolf Heberle, "The Mainsprings of Southern Urbanization," in Vance and Demerath, *The Urban South*, 6–23.

<sup>81</sup> *Alexandria Gazette*, February 7, 1854. Also see George Fitzhugh, *Cannibals All! or, Slaves without Masters* (New York, 1857), 59, and *Sociology for the South: or, The Failure of Free Society* (New York, 1854), 136, 141–42; and Buckner H. Payne, "Contests for the Trade of the Mississippi Valley," *De Bow's Review*, 3 (1847): 98.



stroy urban space and environment—from the railroad tracks dominating the central business districts to the once-beautiful rivers.<sup>82</sup> The economic accomplishments of the New South and its sunbelt offspring have barely changed, or have sometimes altered for the worse, region and city. Progress and tradition, like region and city, are bound together. The battle with colonialism has been, as Pat Watters noted, a “misguided effort of the South to catch up to something that was essentially sorry and shabby in the rest of America.”<sup>83</sup>

THE PROMISING FUTURE that social scientists predicted for the South as a result of urbanization remains elusive. The prophecies have foundered because they assumed that Southern cities were distinct from their region. They were—and are—not. Since the eighteenth century, Southern cities were inextricably tied to, and in some aspects indistinguishable from, the Southern countryside. What Southern cities did not do for the region in the twentieth century is a measure of the continued strength of this connection. Rural values dominated Southern cities because rural people inhabited Southern cities. Evangelical Protestantism fired the souls of frontiersmen and townsmen alike. Cities adopted biracialism wholeheartedly. Were it not for the persistence of the primary victims of the biracial society—the blacks—and the timely, if somewhat halting, assistance of the federal government, urban society *per se* would not have removed many of the obstacles that biracialism signified for blacks. Resistance to integration was, perhaps, more sophisticated in Southern cities than in the countryside, but it was equally, if not more, effective. Finally, there were hopes that Southern cities would lead the region to economic parity. But the cities, rather than becoming regional leaders, are lagging behind regional growth.<sup>84</sup> In addition, the Southern city still performs a secondary role in the national economy, especially in the area of the capital investment and financial services.

Ruralism, race, and colonialism have always characterized a distinctive region and its cities, although these factors and their impact upon urbanization have not been immutable over time. The shift to mechanized agriculture, the growth of modernism in religion, the evolution of biracialism from slavery to exclusion to segregation to integration, and the altered balance in the national economy since the Depression and World War II have affected migration patterns, ways of thought, racial interaction, and capital accumulation in the region—and, hence, urbanization. Nevertheless, these regional elements persist and have collectively produced a particular and limited urbanization that exists in the region to this day. The regional model of urbanization advanced in this essay could provide a helpful framework for further study of the linkages be-

<sup>82</sup> On the continued exploitation of labor and land, see Watters, *The South and the Nation*, 94–97; Lupsha and Siembieda, “The Poverty of Public Services in the Land of Plenty,” 176–77; David C. Perry and Alfred J. Watkins, “People, Profit, and the Rise of the Sunbelt Cities,” in Perry and Watkins, *The Rise of the Sunbelt Cities*, 277–305; and the *New York Times*, May 12, 1978.

<sup>83</sup> Watters, *The South and the Nation*, 530.

<sup>84</sup> See Gurney Breckenfeld, “Refilling the Metropolitan Doughnut,” in Perry and Watkins, *The Rise of the Sunbelt Cities*, 238–39.

tween these elements and urbanization over time. In addition, the model should prove useful in relating the distinctive characteristics of Southern urbanization to urban growth in cities and regions outside of North America. Moving from a world view back to Atlanta, the regional model eliminates the incongruity of Pat Watters's bucolic neighborhood set peacefully in the shadows of those imposing downtown skyscrapers. The Southern city and the South sprang from the same soil, sheltered the same people, and suffered the same burdens—both self-inflicted and superimposed.

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## Review Essay

# The Contours of Southern Progressivism

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DEWEY W. GRANTHAM

THE APRIL 1946 ISSUE of the *North Carolina Historical Review* contained a pioneering article by Arthur S. Link on "The Progressive Movement in the South, 1870-1914." While noting that most writers "ignore the progressive movement in the South altogether," Link contended that the Southern states were the scene of "a far-reaching progressive movement."<sup>1</sup> At the time the young Princeton historian wrote these words, he had virtually no scholarly literature on which to rely, particularly for developments of the early twentieth century. This was still largely true five years later when C. Vann Woodward's interpretation, "Progressivism—For Whites Only," appeared as chapter 14 of his *Origins of the New South, 1877-1913* (1951).<sup>2</sup> Except for a few scholarly articles and a handful of monographs on such topics as child labor reform and prohibition, Woodward was forced to quarry his building materials from primary sources. But the situation was already beginning to change.

During the 1950s the Progressive era in the South became a new historical frontier. The field attracted a growing number of historians, especially young scholars stimulated by Woodward's seminal volume, and their research merged into the larger historiographical assault on the Age of Reform in the United States. Some of the products of this research are apparent in George B. Tindall's *The Emergence of the New South, 1913-1945* (1967) and in other general studies published by Hugh C. Bailey in 1969 and Jack Temple Kirby in 1972.<sup>3</sup> In the preface to an edition of *Origins of the New South* that appeared in 1971, Woodward spoke of "the outpouring of historical scholarship" during the past twenty years, a statement amply substantiated in the 112-page bibliographical essay

This essay is a revision of a paper presented at the Forty-Sixth Annual Meeting of the Southern Historical Association, held in Atlanta, Georgia, November 1980. The author appreciates the criticisms and suggestions of William F. Holmes and Richard Lowitt, the commentators at the SHA session; of his colleague V. Jacque Voegeli; and of the anonymous readers who evaluated the manuscript for the *American Historical Review*. The author alone is responsible for any errors or deficiencies in the essay as published here.

<sup>1</sup> Link, "The Progressive Movement in the South, 1870-1914," *North Carolina Historical Review*, 23 (1946): 172-73.

<sup>2</sup> Woodward, *Origins of the New South, 1877-1913* (Baton Rouge, 1951), 369-95.

<sup>3</sup> Tindall, *The Emergence of the New South, 1913-1945* (Baton Rouge, 1967); Bailey, *Liberalism in the New South: Southern Social Reformers and the Progressive Movement* (Coral Gables, Fla., 1969); and Kirby, *Darkness at the Dawn: Race and Reform in the Progressive South* (Philadelphia, 1972).

prepared for the new edition by Charles B. Dew.<sup>4</sup> The number of books, articles, and essays on the Progressive era in the South continued to mount during the 1970s. Evidence of this historiographical vitality can be found in the selected bibliography published annually in the *Journal of Southern History* and in almost any recent volume of a Southern state's historical journal.

The scholarly writings on the Progressive era in the South are so extensive that it is now possible to think in terms of a comprehensive historical synthesis. This essay attempts to outline the dimensions of such a synthesis. In sketching the contours of Southern progressivism,<sup>5</sup> the essay addresses several questions. First, what were the origins of the progressive impulse in the South? Second, who were the Southern progressives and what were their social values? Third, what were the most important progressive campaigns in the Southern states and what pattern did they assume? Fourth, to what extent did these regional movements eventually become national in orientation? And what effect did Woodrow Wilson's first administration and World War I have on Southern progressivism? Fifth, what of the aftermath of progressivism in the South? Did it have an enduring impact on Southern politics and social attitudes? Last, what are some of the needs and opportunities for further research and writing on the Progressive era in the South?

PERHAPS THE MOST FUNDAMENTAL of the dynamics that contributed to Southern progressivism were changes that had occurred by the end of the nineteenth century in the social landscape of the region, particularly the coming of industry, increasing urbanization, and the growing importance of a new middle class made up of business and professional groups. The increasing economic diversification of the South brought with it what one observer described as "radical changes in the social tendencies" of the section's inhabitants. A spirit of commercialism had become pervasive, and "business" was exalted as never before. Distinct "capitalist" and "laboring" classes were emerging. Social differentiation based on wealth and business success was becoming more pronounced, especially in the cities, and institutions like the church were less "democratic" than in earlier years.<sup>6</sup> The varied forces of economic and social change disrupted many established patterns of life in the South, threatened the stability of the countryside and small town, and precipitated a new awareness of human problems and needs. According to a Vanderbilt professor, writing in 1909, "The most capable business men, lawyers, doctors and preachers are practically all leaving the country for the town and city." This meant that "the great centers of life and influence and authority" were shifting from the country to the city, and

<sup>4</sup> Woodward, *Origins of the New South, 1877-1913* (Baton Rouge, 1971), vii; and Dew, "Critical Essay on Recent Works," *ibid.*, 517-628.

<sup>5</sup> The title of this essay was suggested by Richard Norman Chapman's "Contours of Public Policy, 1939-1945" (Ph.D. dissertation, Yale University, 1976).

<sup>6</sup> Gus W. Dyer, "Social Tendencies in the South," in Julian A. C. Chandler *et al.*, eds., *The South in the Building of the Nation*, 10 (Richmond, 1909): 665-66. Also see Holland Thompson, "Some Effects of Industrialization in an Agricultural State," *South Atlantic Quarterly*, 4 (1905): 75.

"as a result the city is more and more setting the pace of and dominating Southern life and Southern thought."<sup>7</sup>

The expanding role of cities in Southern life brought notable social changes. The growth of this urban South mirrored the industrialization of the region, but its implications went far beyond the organizing and servicing of the economy.<sup>8</sup> The city sorted out people along economic and social lines, facilitated the formation of functional organizations, and fostered a heightened concern for social order, stability, and efficiency. New social types were especially prominent in the South's developing cities, which provided the setting for what Rupert B. Vance later referred to as "a hustling, urban type—the rising merchants, lawyers, and doctors."<sup>9</sup> A new and growing class of supply merchants and bankers provided credit for farmers. In the larger cities and favorably located rural communities a group of industrial entrepreneurs came into existence, mainly in lumber, food, and textile industries.<sup>10</sup> Many small merchants, salesmen, technicians, and clerical workers entered the South's amorphous but expanding middle class.

The emerging urban-industrial system, in the South as elsewhere, demanded a host of services and skills. These were provided by traditional professions such as law, medicine, engineering, education, and journalism and by a wide range of newer professions and specialties in industry, the service trades, public administration, public health, social work, and so on. The vocational and professional concerns of these increasingly differentiated and specialized groups tended both to isolate them from their local communities and to encourage their organization on the basis of function and skill. Industrialists and businessmen came together in trade associations, merchant organizations, bankers' groups, and chambers of commerce. The various professions either reorganized older associations or created new ones for the purpose of elevating professional standards and restricting admission. Other interest groups reflected common social characteristics such as race or ethnicity or common attitudes on particular social issues such as the regulation of community morals.<sup>11</sup> These proliferating organizations, representing the developing middle segment of Southern society, provided an indispensable matrix for the growth of progressivism in the South. They turned to collective action, not only to obtain entrepreneurial and professional advantages but also to help control the social environment in which they operated.

Social reform in the early twentieth-century South was also rooted in the idea

<sup>7</sup> Dyer, "Social Tendencies in the South," 664–67.

<sup>8</sup> See Blaine A. Brownell, "The Urban South Comes of Age, 1900–1940," in Brownell and David R. Goldfield, eds., *The City in Southern History: The Growth of Urban Civilization in the South* (Port Washington, N.Y., 1977), 123–58.

<sup>9</sup> Vance, *Human Factors in Cotton Culture: A Study in the Social Geography of the American South* (Chapel Hill, 1929), 63.

<sup>10</sup> Rudolf Heberle, "The Changing Social Stratification of the South," *Social Forces*, 38 (1959): 45–46.

<sup>11</sup> For a suggestive discussion of the new middle class in the United States in the late nineteenth and early twentieth centuries, see Robert H. Wiebe, *The Search for Order, 1877–1920* (New York, 1967), chap. 5. And, for an excellent analysis of economic and social interest groups in one Southern city, see Carl V. Harris, *Political Power in Birmingham, 1871–1921* (Knoxville, 1977), 39–56.



of Southern progress. The idea was inherent in the longing for economic development and regional rehabilitation. It found expression in many ways, most notably in the creed of the New South. The forward-looking ideas of the New South advocates embodied a compelling vision of regional progress. The benefits to be derived from industrialization seemed boundless. The spokesmen for industrialism in Tennessee, for example, "equated prosperity, progress, and civilization with smoking factory chimneys, booming cities, and rising indices of industrial production."<sup>12</sup> An early student of Southern industrialization in this period concluded,

All of our social gains in the South have been associated with the advance of industry—employment for the poor whites, urban growth with all the activity this implies, sound banking, establishment of a wage system, greater productivity of wealth and its more even distribution, larger tax yields, better schools and roads, improvement of farming methods, and the growth of many governmental services.<sup>13</sup>

Economic advances promised to bring the South "a larger point of view," help diminish "prejudice and emotionalism in southern life," and further national integration.<sup>14</sup>

The idea of Southern progress was promoted not only by the concept of economic development but also by a group of critics who wanted to improve life in the region by reforming various institutions and practices. The new social criticism, more restrained and more hopeful than the searing agrarian indictment of the 1890s, began to have an effect early in the twentieth century. Suddenly, it seemed, ministers, women, professors, writers, and publicists were singling out an assortment of evils in Southern life: in farm conditions, factory work, corporate practices, political life, and so forth.<sup>15</sup> The critics included a liberal sprinkling of crusading journalists, a group of educators and scholars who had recently come out of the new graduate schools, a handful of socially conscious ministers, Southern expatriates like Walter Hines Page and William Garrott Brown, and an occasional politician, usually a state legislator. Some of the critics began to complain about the intolerance—even the tyranny—of public opinion in a section where, as one writer observed, everything must conform to "the Democratic platform, the Daughters of the Confederacy, old General So-and-So, and the Presbyterian creed."<sup>16</sup>

Nevertheless, there was an optimistic note in most of this social commentary. "Here," wrote a contributor to the *South Atlantic Quarterly* in 1905, "is the su-

<sup>12</sup> Constantine G. Belissary, "The Rise of the Industrial Spirit in Tennessee, 1865–1885" (Ph.D. dissertation, Vanderbilt University, 1949), vi. Also see John Spencer Bassett, "How Industrialism Builds Up Education," *World's Work*, 8 (1904): 5030–31.

<sup>13</sup> Broadus Mitchell, "Growth of Manufacturing in the South," *Annals of the American Academy of Political and Social Science*, 153 (1931): 23–24.

<sup>14</sup> Edward K. Graham, "Culture and Commercialism," *South Atlantic Quarterly*, 7 (1908): 121–23.

<sup>15</sup> Herbert J. Doherty, Jr., "Voices of Protest from the New South, 1875–1910," *Mississippi Valley Historical Review*, 42 (1955): 45–66; Bruce Clayton, *The Savage Ideal: Intolerance and Intellectual Leadership in the South, 1890–1914* (Baltimore, 1972); and Tennant S. McWilliams, *Hanniss Taylor: The New Southerner as an American* (University, Ala., 1978).

<sup>16</sup> Walter Hines Page, "The Rebuilding of Old Commonwealths," *Atlantic Monthly*, 89 (1902): 656.

preme opportunity of the Southern newspaper, the Southern college, and Southern criticism of today, to learn from the records of the past the essentials of human progress and to bring these lessons of life to bear on the solution of our own particular problems."<sup>17</sup> A few years later a Southern progressive declared, with considerable satisfaction, that the South had become "a laboratory for the study of sociological forces." The region was "a-making," he wrote, citing its rapid material development, the way its educational system was "democratizing society," the retreat of Southern sectionalism and the "revival of loyalty to the Nation," the "statesman-like way" religious agencies were adjusting to the demands of social change, the "rare insight and initiative" Southern women were demonstrating in furthering social changes, the breaking up of "political crystallization," and the emergence of industrial leaders as "a new power in public affairs." Withal, the instincts of the masses remained "sound and conservative," thus preserving "the ideals of personal honor," and "the traditions of loyalty to home and state" continued "to enrich present forces in Southern life."<sup>18</sup>

Another reform dynamic was incorporated in the growth of a more vigorous humanitarian spirit in the South. The church was the major source of this social compassion. Although Southern Protestantism did far more to conserve than to undermine the dominant culture in the region, Southern churches were agents of change—and reform—as well as of continuity. An impressive number of ministers stood in the vanguard of social reform in the South. The clergy spoke out with greater frequency against social evils, religious bodies showed a deepening interest in the improvement of social conditions, and all of the major Protestant denominations established social action agencies of one kind or another. As Kenneth K. Bailey has written of the three leading white Protestant churches in the South, "Absorbed at the turn of the century in evangelism and little mindful of social needs beyond blue laws and prohibition, they emerged during the next fifteen years as advocates of social justice, proclaiming the Christian obligation to fashion Christ's kingdom on earth."<sup>19</sup> Christian faith continued to center in the gospel of personal redemption, but the churches succeeded in giving a moral-religious tone to much of the region's reformism. The social justice activities of women in the South took shape, appropriately enough, in "church work," particularly in the women's missionary societies and the Woman's Christian Temperance Union. Religion remained "a central aspect" in the lives of many Southern women at the turn of the century, and, as Anne Firor Scott has

<sup>17</sup> William Preston Few, "Southern Public Opinion," *South Atlantic Quarterly*, 4 (1905): 4.

<sup>18</sup> Samuel C. Mitchell, Preface, Introduction, in Chandler *et al.*, *The South in the Building of the Nation*, xix-xx, xxvii.

<sup>19</sup> Bailey, *Southern White Protestantism in the Twentieth Century* (New York, 1964), 42. Also see John Lee Eighmy, *Churches in Cultural Captivity: A History of the Social Attitudes of Southern Baptists* (Knoxville, 1972), 37-87; Henry Y. Warnock, "Moderate Racial Thought and Attitudes of Southern Baptists and Methodists, 1900-1921" (Ph.D. dissertation, Northwestern University, 1963); Wayne Flynt, "Dissent in Zion: Alabama Baptists and Social Issues, 1900-1914," *Journal of Southern History* [hereafter, *JSH*], 35 (1969): 523-42; Harry G. Lefever, "The Involvement of the Men and Religion Forward Movement in the Cause of Labor Justice, Atlanta, Georgia, 1912-1916," *Labor History*, 14 (1973): 521-35; and Charles R. Wilson, "Baptized in Blood: Southern Religion and the Cult of the Lost Cause, 1865-1920" (Ph.D. dissertation, University of Texas, Austin, 1977).

pointed out, its form gradually changed from "intense personal piety to a concern for the salvation of the heathen and for social problems."<sup>20</sup>

The origins of Southern progressivism can also be traced to the changing political scene and the transformation of Southern politics in the late nineteenth century. The most distinctive attribute of political affairs in the South at the turn of the century was the overwhelming dominance of the Democratic party. Despite the fissures in the section's political solidarity caused by the Populist revolt of the 1890s, Democratic supremacy in the states from Virginia to Texas was more secure than ever before. This meant that the competition for political leadership and public office, the formulation and discussion of political issues, the fate of reform movements, and the outcome of legislative action were all decided within the confines of one-party politics. This simple fact had a profound influence on the nature of Southern progressivism.

Several features of the South's altered political system were especially significant in shaping Southern progressivism. One of these was the drastic shrinkage of the electorate as a result of the restructuring of Southern politics in the 1890s and early 1900s. Disfranchisement and restrictive election laws not only deprived most black Southerners of the ballot but also, in conjunction with the persistence of poverty, illiteracy, and cultural barrenness, sharply limited the political involvement of millions of whites.<sup>21</sup> A second important feature of Southern politics in the Progressive period was the direct primary and other democratic devices, whose adoption introduced new elements into the region's politics and promised changes in the complexion of state government. These innovations contributed to a third notable development in the politics of the South: the vigorous competition in the Democratic party and the prevalence of bifactional cleavages in most Southern states. The implications of these developments for social reform were ambiguous, but such changes facilitated the emergence of interest-group politics and enhanced the political influence of middle-class organizations and professional groups, whether in mobilizing mass support or bringing pressure to bear on legislators and other public officials. Although the remodeled system was restrictive and undemocratic, it nonetheless provided a setting for insurgent political campaigns, for broad appeals to the public, for the open discussion of issues, and for the organization and conduct of reform movements.

The political context in which Southern progressivism developed was also af-

<sup>20</sup> Scott, "Women, Religion, and Social Change in the South, 1830-1930," in Samuel S. Hill, Jr., et al., *Religion and the Solid South* (Nashville, Tenn., 1972), 93. Also see Noreen Dunn Tatum, *A Crown of Service: A Story of Woman's Work in the Methodist Episcopal Church, South, from 1878-1940* (Nashville, Tenn., 1960); and Anne Firor Scott, "The 'New Woman' in the New South," *South Atlantic Quarterly*, 61 (1962): 473-83.

<sup>21</sup> Francis G. Caffey, "Suffrage Limitations at the South," *Political Science Quarterly*, 20 (1905): 53-67; Ralph Clipman McDaniel, *The Virginia Constitutional Convention of 1901-1902* (Baltimore, 1928); Paul Lewinson, *Race, Class, & Party: A History of Negro Suffrage and White Politics in the South* (New York, 1932), 79-123, 194-217; Malcolm Cook McMillan, *Constitutional Development in Alabama, 1798-1901: A Study in Politics, the Negro, and Sectionalism* (Chapel Hill, 1955), 263-359; Allie Bayne Windham Webb, "A History of Negro Voting in Louisiana, 1877-1906" (Ph.D. dissertation, Louisiana State University, 1962); and J. Morgan Kousser, *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880-1910* (New Haven, 1974), 51-53, 72-76, 91-92, 126, 155, 166, 170, 175, 206-07, 230, 246-47, 251-57, 261.

fectured by populism. In appealing for solidarity in the white community, Democratic leaders were moved to make some concessions to the agrarian reformers, particularly in their rhetorical obeisance to the ideal of popular government. There was, to be sure, an indigenous reformism in the Democratic party of the various Southern states.<sup>22</sup> Many Southern Democrats were genuine advocates of reform and were not motivated by a desire merely to propitiate Populists.<sup>23</sup> But populism challenged Democratic commitments and served as a catalyst for selective change in Southern politics. Populist proposals for stringent railroad regulation, liberal agricultural credit, abolition of convict leasing, and support of public education became part of the reform agenda of the region's progressives. Southern Democrats increasingly accepted the Populist concept of the positive state—of a more active governmental role in promoting economic growth and protecting society—and this notion found fertile ground in the new climate of interest-group politics. A goodly number of former Populists became leaders in the Democratic party, and agrarian radicals often discovered compatible allies in the Bryanized wing of the party. The persistence of agrarian radicalism also seems to have contributed to the vigorous intraparty factionalism that developed in the one-party South during this period.

Politics, of course, constituted an essential medium for the waging of progressive campaigns. State and local governments were the primary agencies for the resolution of conflicts in the community and for the regulation of business practices and social behavior, as well as the source of public services. The South's economic growth and diversification increased the demands on state and local governments for franchises, services, and regulations. The expanding cities and towns were confronted with especially troublesome problems, which often required action by state legislatures. With the enhanced role of government came a dramatic enlargement in the part played by economic and professional organizations in the formulation and enactment of public policy. Chambers of commerce, freight bureaus, farmers' organizations, labor unions, professional associations, and scores of other groups were soon participating in local and state politics throughout the region.

All of these tendencies—social change, the emergent ideology of Southern progress, a broadening humanitarianism, and the transformation of politics—converged around the turn of the century to provide a favorable setting for Southern progressivism. The divisions and frustrations of the 1890s formed a somber backdrop for the sunnier outlook of the new decade. The ending of the

<sup>22</sup> See, for example, Carl V. Harris, "Right Fork or Left Fork? The Section-Party Alignments of Southern Democrats in Congress, 1873-1897," *JSH*, 42 (1976): 471-506.

<sup>23</sup> For the diversity of scholarly thought on this subject, see, among others, Albert D. Kirwan, *Revolt of the Rednecks: Mississippi Politics, 1876-1925* (Lexington, Ky., 1951); Robert C. Cotner, *James Stephen Hogg: A Biography* (Austin, 1959); Henry C. Dethloff, "Populism and Reform in Louisiana" (Ph.D. dissertation, University of Missouri, 1964); Allen W. Jones, "Political Reforms of the Progressive Era," *Alabama Review*, 21 (1968): 173-94; William Ivy Hair, *Bourbonism and Agrarian Protest: Louisiana Politics, 1877-1900* (Baton Rouge, 1969); Alwyn Barr, *Reconstruction to Reform: Texas Politics, 1876-1906* (Austin, 1971); Roger L. Hart, *Redeemers, Bourbons, & Populists: Tennessee, 1870-1896* (Baton Rouge, 1975); Danney Goble, *Progressive Oklahoma: The Making of a New Kind of State* (Norman, Okla., 1980); and Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976), 214-17, 328-43, 520-21, 534-37.

economic depression lifted Southern spirits, and talk of the South's imminent agricultural and industrial expansion assumed a new animation. With the passing of the political turmoil of the 1890s, the public mood became more relaxed. The threat to the integrity of the "Southern" community had faded away. Democratic hegemony having been re-established, politicians paid greater heed to the widespread revulsion against the political corruption and electoral fraud of earlier years. The "race question" was apparently being settled through disfranchisement and segregation laws. The possibility of outside interference in the politics and social arrangements of the South, despite campaign rhetoric, seemed increasingly remote, and "Northern" opinion was, for the most part, agreeably acquiescent. The South's enthusiastic support of the Spanish-American War appeared to symbolize the final achievement of intersectional harmony and national integration.

IDENTIFYING THE SOUTHERN PROGRESSIVES continues to pose a challenge to historians of reform movements in the early twentieth-century South. In his 1946 essay Arthur S. Link suggested that after 1900 the leadership of the Progressive movement passed from "the hands of the farmers to progressive editors, politicians, and other urban groups." As a general rule, Link wrote, the progressives found recruits in "the middle classes of the South among the more prosperous farmers, small businessmen, school teachers, editors, and other professional groups."<sup>24</sup> C. Vann Woodward, in *Origins of the New South*, advanced a similar interpretation. "Southern progressivism," in his view, "was essentially urban and middle class in nature, and the typical leader was a city professional man or businessman, rather than a farmer." In pushing for reform, Woodward added, small businessmen and the urban middle class "joined hands with the discontented farmers."<sup>25</sup> Subsequent interpretations have generally confirmed the Link-Woodward thesis. George B. Tindall, in *The Emergence of the New South* (1967), located the wellsprings of the progressive cause in "the aspirations of the middle-income groups to own and develop productive property"; progressivism in the South drew strength from "merchants, mechanics, farmers, small manufacturers, and the brokers and factors who serviced the farm economy."<sup>26</sup> Writing in 1979, Willard B. Gatewood described the progressive coalitions as including "businessmen, professionals, certain agricultural interests, and a medley of social uplift agents."<sup>27</sup> These judgments, while persuasive, were for the most part based on impressionistic evidence; there are as yet few studies that convincingly identify rank-and-file supporters and opponents of the various progressive campaigns in the South.

Although the composition of the progressive coalitions in the South remains

<sup>24</sup> Link, "The Progressive Movement in the South," 179-80.

<sup>25</sup> Woodward, *Origins of the New South* (1971), 371.

<sup>26</sup> Tindall, *The Emergence of the New South*, 6.

<sup>27</sup> Gatewood, "Progressivism," in David C. Roller and Robert W. Twyman, eds., *The Encyclopedia of Southern History* (Baton Rouge, 1979), 1006. For other examples, see Bailey, *Liberalism in the New South*, 11-12, 58; Sheldon Hackney, *Populism to Progressivism in Alabama* (Princeton, 1969), 126, 138, 209, 254, 324-25, 331; and Kirby, *Darkness at the Dawning*, 2-3, 26-56.



inadequately analyzed, it is clear that many of them involved what Sheldon Hackney has called "the politics of pluralistic interest groups."<sup>28</sup> These groups were primarily concerned with their own survival and competitive position, but their increasing resort to collective action made them a major consideration in reform politics. The commercial-civic elites and other organized elements often opposed social reforms, of course, and their character differed from state to state. Urban influences were obviously stronger in Tennessee than in Mississippi. Cultural traditions were more significant in Virginia than in Oklahoma politics. Farmer and labor groups were better represented in the politics of the Southwestern states than in the states of the Southeast. In general, however, the politics of pluralistic interest groups reflected the strength and vigor of the new commercial and professional classes centered in the region's cities. These groups included lawyers, editors, teachers, ministers, doctors, businessmen, agricultural scientists, demonstration agents, city planners, labor leaders, social workers, YMCA directors, railroad commission experts, and legislative lobbyists. These middle-class men and women were the quintessential Southern progressives.

Nevertheless, rural Southerners played an important part in the South's progressivism, particularly in legislative enactments and support of such causes as disfranchisement, public education, railroad regulation, and prohibition. One aspect of this question, the relationship between populism and progressivism in the South, has evoked differing interpretations. Raymond H. Pulley has found no continuity between the two movements in Virginia, arguing instead that progressivism there developed as "a direct reaction" to populism.<sup>29</sup> In the case of Alabama, Hackney has contended, progressivism was an alternative to populism that represented "a substantially different reaction by a separate set of men to the same enemy Populism faced—the dominant industrial wing of the Democratic Party."<sup>30</sup> Tindall, however, though not directly concerned with the roots of Southern progressivism, has characterized it as "an amalgam of agrarian radicalism, business regulation, good government, and urban social justice reforms."<sup>31</sup> Kirby has depicted progressivism in the South as "dichotomous" and "multidirectional," but he has also suggested that "the most potent force for southern reform lay in the frustrations and yearnings of the rural and small town masses." And he has cited the pervasive antimonopoly spirit among Southerners as "the great connecting link between the rural protests of the nineteenth century and those of the twentieth."<sup>32</sup> More substantial agreement on

<sup>28</sup> Hackney, *Populism to Progressivism in Alabama*, 209.

<sup>29</sup> Pulley, *Old Virginia Restored: An Interpretation of the Progressive Impulse, 1870-1930* (Charlottesville, 1968), ix, 56, 93.

<sup>30</sup> Hackney, *Populism to Progressivism in Alabama*, 122. By contrast, a recent study of Georgia Populists argues for a strong link between populism and progressivism in that state. See Barton Carr Shaw, "The Wool-Hat Boys: A History of the Populist Party in Georgia, 1892 to 1910" (Ph.D. dissertation, Emory University, 1979).

<sup>31</sup> Tindall, *The Emergence of the New South*, 32.

<sup>32</sup> Kirby, *Darkness at the Dawning*, 1-3, 26-27. James R. Green has found a close relationship between populism and socialism in the states of Texas, Oklahoma, Louisiana, and Arkansas, although he has concluded that the two movements represented "significantly different constituencies on the basis of rather different ideologies." The Socialist party in the Southwest, he wrote, showed greater continuity with the People's party in leadership than in membership. Green, *Grass-Roots Socialism: Radical Movements in the Southwest, 1895-1943* (Baton Rouge, 1978), xii-xiii, 24.

this important question must await the completion of additional studies like that of Hackney on Alabama. Meanwhile, historians should be wary of exaggerating the discontinuity between the reform movements of the 1890s and those of the early twentieth century. The concept of "a continuing and potent agricultural influence," to borrow the phrase James Tice Moore applied to the Age of the Southern Redeemers,<sup>33</sup> may have considerable validity in the case of the Progressive era in the South.

Southern progressives were no doubt a rather disparate collection of social reformers, but they were unified in some measure by common goals and social values. They shared a yearning for a more orderly and cohesive community.<sup>34</sup> Such a community, they believed, was a prerequisite for economic development and material progress. Its realization depended upon the effective regulation of society in the interest of ethical business practices and good government, and in the elimination of political corruption, machine politics, and the insidious power of large corporations and other special interests. Social controls were also indispensable to the preservation of moral values, to the purification of social institutions, and to the protection of men from their own weaknesses. Optimistic about future prospects but alarmed by the tensions and turmoil that pervaded the South in the late nineteenth century, Southern progressives looked toward the creation of a clearly defined community that would accommodate a society differentiated by race and class but one that also possessed unity, cohesion, and stability. The search for community may explain the reformers' obsession with the virtues of rural life and with means of improving it.

Progressives in the South, like other American reformers in this period, talked about the virtues of "the people," identified morality with majority rule, and urged the desirability of preserving and expanding traditional democratic principles. But, characteristically, their concept of democracy was limited. The ideal they invoked was that of "*Herrenvolk* democracy"—a democratic society for whites only.<sup>35</sup> Racial segregation and black disfranchisement, some white leaders claimed, were the touchstones through which the distinctions separating white men could be softened, white unity maintained, and a broader white democracy achieved. But well-to-do and middle-class Southerners, including many social reformers, often revealed a deep distrust of the masses, whether black or white.<sup>36</sup> While the social critics and reformers in the early twentieth-century South worked for the education and uplift of the common man, they were fully aware of his prejudices and narrow-mindedness, of his extreme sensitivity to criticism, obsession with the race question, and susceptibility to senti-

<sup>33</sup> Moore, "Redeemers Reconsidered: Change and Continuity in the Democratic South, 1870-1900," *JSH*, 44 (1978): 362.

<sup>34</sup> See, for example, Pulley, *Old Virginia Restored*, 49.

<sup>35</sup> See George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817-1914* (New York, 1971), 256-319.

<sup>36</sup> If progressivism had a general theme in the South, J. Morgan Kousser has suggested, it was hardly "democracy" or "the greatest good for the greatest number" but rather the stabilization of society in the interest of long-established powers—at the expense of the section's poorer elements; *The Shaping of Southern Politics*, 230. This interpretation slights the influence of other social values, including humanitarian concern and social justice, in the motivation of Southern progressives.

mentalism. Thus, Southern progressives demonstrated a proclivity toward paternalistic solutions in dealing with many social problems. It is significant that progressives sometimes coupled education, which they emphasized as an instrument of material progress and social control, with the need to cleanse the political process and limit participation to those who were prepared for responsible citizenship.<sup>37</sup>

For all of their emphasis on social order and their faith in social controls, many Southern progressives revealed a strong commitment to social justice and the amelioration of human suffering in their communities. A growing number of Southerners were genuinely worried about the consequences of industrialization for ordinary people, aware of the increasing need for social services, and sensitive to the social roles and responsibilities opening up to them as part of an emerging class of trades people, professionals, and experts. The creative response of Southern women to the plight of the poor and disadvantaged was especially notable in the South's uplift campaigns. The cause of social justice in the South was also fostered by the monetary and moral support of Northern philanthropists during the Progressive era.

Most Southern progressives were convinced that much of the South's social distress could be relieved or prevented through economic development. They accepted, like so many contemporary Southerners, the basic assumptions of the New South program of regional progress through rapid economic growth, industrialization, and a more diversified economy. These objectives led directly to an emphasis on efficiency and rationality not only in the production of goods but also in such areas as education, the treatment of criminals, race relations, and the prohibition of alcoholic beverages. The theme of social efficiency was prominent, for instance, in the efforts of Southern progressives to improve farm life, in their approach to industrial labor, in their municipal reforms and innovations, and in their advocacy of a larger role for state governments as promoters, regulators, and arbiters. The search for efficiency in these diverse areas of Southern society, while not exclusively or even primarily a manifestation of social reform, was nonetheless a significant aspect of the progressive mentality and program in the South.

These social values—order, morality, benevolence, efficiency, and development—were not separate categories of progressive concern. Rather they were, as Hackney has written of the Alabama progressives, “interrelated facets of a single, economically self-interested, ethically shaped, middle-class attitude toward life.”<sup>38</sup> These areas of social concern and commitment, moreover, were mutually reinforcing, and in seeking to give them effect Southern progressives began to develop an expanded concept of governmental responsibilities. They moved beyond the regulatory state to advocate a broadening array of public

<sup>37</sup> See, for example, Claude H. Nolen, *The Negro's Image in the South: The Anatomy of White Supremacy* (Lexington, Ky., 1967), 118–52; and Louis R. Harlan, “The Southern Education Board and the Race Issue in Public Education,” *JSH*, 23 (1957): 189–202.

<sup>38</sup> Hackney, *Populism to Progressivism in Alabama*, 138.

services. In other words, Southern progressivism became "a movement for positive government."<sup>39</sup>

IDENTIFYING THE MAJOR MOVEMENTS that comprised Southern progressivism and categorizing them on the basis of their fundamental purposes may be helpful at this point. The reform movements were often interrelated, and in many cases the same reformers were prominent in several progressive campaigns. In Virginia and South Carolina, for example, the educator Samuel Chiles Mitchell was an ardent champion of public schools, child labor reform, prohibition, public health, better roads, improved race relations, and public welfare programs.<sup>40</sup> Furthermore, the progressive endeavors were inspired by a variety of motives; thus, the classification suggested in this essay is somewhat arbitrary. One group of reform efforts appears to have been primarily concerned with governmental regulation and the imposition of social controls in troublesome areas such as race relations. The race settlement of the 1890s and early 1900s was one such manifestation of Southern progressivism. The white consensus that developed during this period reflected a widespread conviction that disfranchisement, segregation, and black proscription not only made up a workable system of racial control but also promised a greater measure of social stability and public calm. Such a milieu, it was said, would make it possible for progressives to address themselves to other pressing social causes. It might also help establish a new national consensus of "enlightened" and "liberal" opinion on the race question.<sup>41</sup>

Prison reform, centering on efforts to abolish the leasing of convicts, to introduce prison farms, and to develop a new system of road work for prisoners, constituted another example of Southern progressivism largely concerned with the application of more efficient social controls.<sup>42</sup> Antimonopolism represented another manifestation of the regulatory impulse among the region's progressives. The movement to control railroads and other large corporations and to destroy their political dominance became a major objective of progressives in many Southern states. Corporate regulation also served as one of the key issues in providing a rallying point for progressive politics in the South.<sup>43</sup> For big busi-

<sup>39</sup> Tindall, *The Emergence of the New South*, 32.

<sup>40</sup> Daniel W. Hollis, "Samuel Chiles Mitchell, Social Reformer in Bleasde's South Carolina," *South Carolina Historical Magazine*, 70 (1969): 20-37.

<sup>41</sup> Kirby, *Darkness at the Dawning*, 4-8; Fredrickson, *The Black Image in the White Mind*, 298-99, 304; Dewey W. Grantham, *The Regional Imagination: The South and Recent American History* (Nashville, Tenn., 1979), 77-106; John William Graves, "Town and Country: Race Relations and Urban Development in Arkansas, 1874-1905" (Ph.D. dissertation, University of Virginia, 1978); and Charles K. Piehl, "White Society in the Black Belt, 1870-1920: A Study of Four North Carolina Counties" (Ph.D. dissertation, Washington University, 1979).

<sup>42</sup> Hilda Jane Zimmerman, "Penal Systems and Penal Reforms in the South since the Civil War" (Ph.D. dissertation, University of North Carolina, 1947); Mark T. Carleton, *Politics and Punishment: The History of the Louisiana State Penal System* (Baton Rouge, 1971); Pete Daniel, *The Shadow of Slavery: Peonage in the South, 1901-1969* (Urbana, Ill., 1972); and William Cohen, "Negro Involuntary Servitude in the South, 1865-1940: A Preliminary Analysis," *JSH*, 42 (1976): 31-60.

<sup>43</sup> See, for example, Maxwell Ferguson, *State Regulation of Railroads in the South* (New York, 1916); William H. Glasson, "The Crusade against the Railroads," *South Atlantic Quarterly*, 6 (1907): 165-76; James F. Doster, *Railroads in Alabama Politics, 1875-1914* (University, Ala., 1957); Albert Neely Sanders, "State Regulation of Public

ness, so often identified with powerful outside interests, entrenched political "machines," unethical business practices, and the destruction of competition, seemed to demand stronger public control no less than did black workers, criminals, and alcoholic beverages.<sup>44</sup> The concentrated attack of Southern governors and legislatures upon railroads and other corporations in 1906 and 1907 soon spent itself, and much of the regulatory force during the next decade found an outlet in the antiliquor crusade, first in the drive for statewide prohibition and subsequently in the campaign for a national law.<sup>45</sup> Prohibition offered a means of moral reaffirmation of traditional values, an assurance of cleaner politics, and a way to employ the power of the state in the pursuit of moral and social progress.

A second significant category of progressive campaigns in the South was dominated by the theme of social justice. One of the principal reform movements in this sphere was devoted to the regulation of child labor.<sup>46</sup> The child-labor campaigns brought the section's social reformers together in a common cause, gave them valuable experience in organizing for reform purposes, and stimulated their interest in the establishment of juvenile courts and programs for the care of dependent children. No aspect of social reform in the South during the Progressive era touched the immediate lives of more of the region's inhabitants than the great educational awakening soon after the turn of the century.<sup>47</sup> Education was the entering wedge and the sustaining focus for unprecedented Northern philanthropy as well as a major element in the rationale of interregional accommodation that flourished during these years. It was almost always viewed by reformers as a redemptive force in the development of a better South. A third area of social justice concern was the organized charity movement. By the end of the first decade of the twentieth century, social welfare in the South had begun to move away from the long-dominant emphasis on re-

Utilities by South Carolina, 1878-1935" (Ph.D. dissertation, University of North Carolina, 1956); Robert Lewis Peterson, "State Regulation of Railroads in Texas, 1836-1920" (Ph.D. dissertation, University of Texas, Austin, 1960); and Daniel Camille Vogt, "Problems of Government Regulation: The Mississippi Railroad Commission, 1884-1956" (Ph.D. dissertation, University of Southern Mississippi, 1980).

<sup>44</sup> See William Garrott Brown, "The South and the Saloon," *Century Magazine*, 76 (1908): 462-66.

<sup>45</sup> For some of the many useful studies of the prohibition movement in the South, see James Benson Sellers, *The Prohibition Movement in Alabama, 1702 to 1943* (Chapel Hill, 1943); Daniel Jay Whitener, *Prohibition in North Carolina, 1715-1945* (Chapel Hill, 1945); Paul E. Isaac, *Prohibition and Politics: Turbulent Decades in Tennessee, 1885-1920* (Knoxville, 1965); C. C. Pearson and J. Edwin Hendricks, *Liquor and Anti-Liquor in Virginia, 1619-1919* (Durham, N.C., 1967); Jimmie Lewis Franklin, "Prohibition in Oklahoma, 1907-1959" (Ph.D. dissertation, University of Oklahoma, 1968); Lewis L. Gould, *Progressives and Prohibitionists: Texas Democrats in the Wilson Era* (Austin, 1973); and James H. Timberlake, *Prohibition and the Progressive Movement, 1900-1920* (Cambridge, Mass., 1963), 73-74, 109-14, 119-20, 123, 164-65.

<sup>46</sup> Elizabeth H. Davidson, *Child Labor Legislation in the Southern Textile States* (Chapel Hill, 1939); Hugh C. Bailey, *Edgar Gardner Murphy: Gentle Progressive* (Coral Gables, Fla., 1968); Herbert J. Doherty, Jr., "Alexander J. McKelway: Preacher to Progressive," *JSH*, 24 (1958): 177-90; and Betty Jane Brandon, "Alexander Jeffrey McKelway: Statesman of the New Order" (Ph.D. dissertation, University of North Carolina, 1969).

<sup>47</sup> See Charles William Dabney, *Universal Education in the South*, volume 2: *The Southern Education Movement* (Chapel Hill, 1936); General Education Board, *The General Education Board: An Account of Its Activities, 1902-1914* (New York, 1915); Rose Howell Holder, *McIver of North Carolina* (Chapel Hill, 1957); Andrew David Holt, *The Struggle for a State System of Public Schools in Tennessee, 1903-1936* (New York, 1938); Horace Mann Bond, *Negro Education in Alabama: A Study in Cotton and Steel* (Washington, 1939); and Louis R. Harlan, *Separate and Unequal: Public School Campaigns and Racism in the Southern Seaboard States, 1901-1915* (Chapel Hill, 1958).



lief of the destitute and to put greater stress on casework, surveys, and organization.<sup>48</sup>

The cause of social justice also included an embryonic movement to ameliorate the conditions of black people in the South. During the Progressive period, the primary concerns of white Southerners, including most social reformers, in their approach to the "race problem" were social efficiency and the means of social control. Yet some whites sought to ease the terrible burden of racial injustice borne by the Negro. Their program, as one scholar has recently observed, was based on "an updated version of paternalism in which whites would offer blacks help, guidance, and protection in exchange for a commitment to the New South values of thrift and hard work, as well as a continued subservience."<sup>49</sup>

There was also a black approach to racial progress and better social conditions. Given sustenance by an emerging Negro middle class, it envisioned "an expanded concept of social justice, a more efficient pattern of living, and a greater emphasis upon local organizations."<sup>50</sup> Blacks, as John Dittmer has written, "built their own institutions behind the walls of segregation, preaching race pride and practicing self-help."<sup>51</sup> Negro civic organizations, boards of trade, public welfare leagues, and community betterment groups, particularly in the larger cities, labored to extract worthwhile concessions in education and other public services from the white system, to secure more adequate correctional facilities for black juveniles, to improve housing and sanitation in Negro areas, and to promote moral conduct, social order, and efficiency in the black community. The community work of Negro club women was a prominent feature of this black progressivism. The glimpses that recent studies provide into the world of social reform among blacks, like those into the world of inarticulate Southern

<sup>48</sup> Frank Dekker Watson, *The Charity Organization Movement in the United States: A Study in American Philanthropy* (New York, 1922), 248, 337-45, 347-58, 361; Howard W. Odum and D. W. Willard, *Systems of Public Welfare* (Chapel Hill, 1925); Lyda Gordon Shivers, "The Social Welfare Movement in the South: A Study in Regional Culture and Social Organization" (Ph.D. dissertation, University of North Carolina, 1935); Elizabeth Wisner, *Social Welfare in the South: From Colonial Times to World War I* (Baton Rouge, 1970); Milton D. Speizman, "The Movement of the Settlement House Idea into the South," *Southwestern Social Science Quarterly*, 44 (1963): 237-46; and Henry D. Shapiro, *Appalachia on Our Mind: The Southern Mountains and Mountaineers in the American Consciousness, 1870-1920* (Chapel Hill, 1978), 133-243.

<sup>49</sup> Daniel Joseph Singal, "Ulrich B. Phillips: The Old South as the New," *Journal of American History*, 63 (1977): 881.

<sup>50</sup> Lester C. Lamon, *Black Tennesseans, 1900-1930* (Knoxville, 1977), 208.

<sup>51</sup> Dittmer, *Black Georgia in the Progressive Era, 1900-1920* (Urbana, Ill., 1977), 50. Also see *ibid.*, 63-65, 147-48, 162, 167, 173-74, 180; August Meier, *Negro Thought in America, 1880-1915: Racial Ideologies in the Age of Booker T. Washington* (Ann Arbor, 1963); James M. McPherson, "The Antislavery Legacy: From Reconstruction to the NAACP," in Barton J. Bernstein, ed., *Towards a New Past: Dissenting Essays in American History* (New York, 1968), 126-57; Lamon, *Black Tennesseans, 1900-1930*, 20-36, 88-109, 207-30; I. A. Newby, *Black Carolinians: A History of Blacks in South Carolina from 1895 to 1968* (Columbia, S.C., 1973); Gerda Lerner, "Early Community Work of Black Club Women," *Journal of Negro History*, 59 (1974): 158-67; Edyth L. Ross, "Black Heritage in Social Welfare: A Case Study of Atlanta," *Phylon*, 37 (1976): 297-307; Alfred Alfonso Moss, Jr., "The American Negro Academy: Voice of the 'Talented Tenth'" (Ph.D. dissertation, University of Chicago, 1977); John Howard Burrows, "The Necessity of Myth: A History of the National Negro Business League, 1900-1945" (Ph.D. dissertation, Auburn University, 1977); Cynthia Griggs Fleming, "The Development of Black Education in Tennessee, 1865-1920" (Ph.D. dissertation, Duke University, 1977); Herbert L. Clark, "The Public Career of James Carroll Napier: Businessman, Politician, and Crusader for Racial Justice, 1845-1940" (D.A. thesis, Middle Tennessee State University, 1980); and Faye W. Robbins, "A World-within-a-World: Black Nashville, 1880-1915" (Ph.D. dissertation, University of Arkansas, 1980).

whites, suggest that much research remains to be done on the intricacies of progressivism in the South.

Social efficiency, especially as it related to economic development, was a motif in several of the reform movements. A multifaceted attack on the ills of Southern agriculture made up one such movement.<sup>52</sup> Despite the frequent invocation of rural values, the effort to rehabilitate the Southern farmer basically entailed programs to improve agricultural techniques, increase production, and raise farm income. Municipal reform was also an important part of Southern progressivism. It embodied all of the major themes of Southern reformism in the early twentieth century—regulation and social control, humanitarian efforts to relieve social distress, and the drive for efficiency.<sup>53</sup> Protective labor measures and other reforms affecting industrial workers provide still another example of the emphasis on social efficiency in Southern progressivism. As a rule, the legislative concerns and social policy preferences of the state labor federations, railroad brotherhoods, and labor councils were compatible with the program and rationale of Southern progressivism.<sup>54</sup>

The progressive campaigns for efficiency in agriculture, municipal government, and industrial labor led to greater emphasis on scientific knowledge, expertise, and effective administration in the public arena. Similar pressures emanated from other reform campaigns, such as the movements for public education, public health, and good roads. The state was increasingly viewed not only as the source of regulatory action but also as the provider of vital new services.<sup>55</sup> This expanded role of government was in harmony with the widespread commitment in the South to economic development and with the entrepreneurial aspirations of diverse groups and specialized organizations representing farmers, industrial workers, and business and professional men and women. The steadily mounting demand for public services formed an important dimension of Southern progressivism.

Perhaps this sketch of reform movements will suggest the varied and overlapping incentives that underlay Southern progressivism. The campaign for

<sup>52</sup> Carl C. Taylor, *The Farmers' Movement, 1620-1920* (New York, 1953); Joseph Cannon Bailey, *Seaman A. Knapp: Schoolmaster of American Agriculture* (New York, 1945); Willard Range, *A Century of Georgia Agriculture, 1850-1950* (Athens, Ga., 1954); Theodore Saloutos, *Farmer Movements in the South, 1865-1933* (Berkeley and Los Angeles, 1960); and Roy V. Scott, *The Reluctant Farmer: The Rise of Agricultural Extension to 1914* (Urbana, Ill., 1970).

<sup>53</sup> See Bradley Robert Rice, *Progressive Cities: The Commission Government Movement in America, 1901-1920* (Austin, 1977), 3-33, 52-109; Howard N. Rabinowitz, "Continuity and Change: Southern Urban Development, 1860-1900," in Brownell and Goldfield, *The City in Southern History*, 92-122; Blaine A. Brownell, "The Urban South Comes of Age, 1900-1940," *ibid.*, 123-58; Harris, *Political Power in Birmingham*; William D. Miller, *Memphis during the Progressive Era, 1900-1917* (Memphis, 1957); and James Weinstein, "Organized Business and the City Commission and Manager Movements," *JSH*, 28 (1962): 166-82.

<sup>54</sup> Herbert J. Lahne, *The Cotton Mill Worker* (New York, 1944); F. Ray Marshall, *Labor in the South* (Cambridge, Mass., 1967); Keith L. Bryant, Jr., "Labor in Politics: The Oklahoma State Federation of Labor during the Age of Reform," *Labor History*, 11 (1970): 259-76; and Melton Alonza McLaurin, *Paternalism and Protest: Southern Cotton Mill Workers and Organized Labor, 1875-1905* (Westport, Conn., 1971).

<sup>55</sup> Joseph Hyde Pratt, "Good Roads Movement in the South," *Annals of the American Academy of Political and Social Science*, 35 (1910): 105-13; Francis R. Allen, "Development of the Public Health Movement in the Southeast," *Social Forces*, 22 (1943): 67-75; and Thomas F. Abercrombie, *History of Public Health in Georgia, 1733-1950* (Atlanta, 1951).

public education, for example, was inspired by the reformers' interest in social order, efficiency, and development as well as in social justice. A similar mixture of motives is apparent in most other progressive endeavors in the South. The reform movements unfolded more or less simultaneously during the first decade of the twentieth century, except for disfranchisement and the race settlement, which were well under way by 1900. Beginning as scattered and loosely organized efforts in the late 1890s and early 1900s, the more significant reform movements soon moved to the creation of state organizations and campaigns.<sup>56</sup> The pace of these campaigns varied from state to state, but in many cases they led to coordinated or parallel activities throughout the South, although the process was occasionally reversed, as in the launching of the Southern education movement. The campaign against child labor, the education crusade, and several other reform movements were set in motion during the first two or three years of the new century. The railroad regulatory campaigns, led by several reform governors, reached their height in 1907, and during the next year or two the prohibition movement enjoyed spectacular success, before losing momentum during the years 1910-13.<sup>57</sup>

Virtually all of the progressive campaigns had assumed a regionwide character by 1910, and they were promoted as "Southern" reforms. A kind of progressive balance had emerged: the claims of numerous social groups were given a hearing and tensions were modulated between classes, races, rural and city dwellers, South and North, tradition and innovation. In other words, progressives were able to create a strong sense of community as a setting for their pursuit of social reform.

By the time Woodrow Wilson assumed the presidency in March 1913, a new stage had arrived in the evolution of Southern progressivism. Reform movements in the Southern states, as in other sections, were increasingly influenced by national organizations, standards, and solutions. This tendency was evident in the formation of the Southern Sociological Congress, a regional civic organization established in Nashville in the spring of 1912. Characterized by a zeal for uplift and an evangelical spirit, the congress was intended to serve as a medium for organizing and coordinating various social reform groups. Its creation re-

<sup>56</sup> The content of Southern progressivism is described in the works cited above. Also see James Aubrey Tinsley, "The Progressive Movement in Texas" (Ph.D. dissertation, University of Wisconsin, Madison, 1953); Joseph Flake Steelman, "The Progressive Era in North Carolina, 1884-1917" (Ph.D. dissertation, University of North Carolina, 1955); Charles Granville Hamilton, "Mississippi Politics in the Progressive Era, 1904-1920" (Ph.D. dissertation, Vanderbilt University, 1958); Alton DuMar Jones, "Progressivism in Georgia, 1898-1918" (Ph.D. dissertation, Emory University, 1963); David Alan Harris, "Racists and Reformers: A Study of Progressivism in Alabama, 1896-1911" (Ph.D. dissertation, University of North Carolina, 1967); Matthew James Schott, "John M. Parker of Louisiana and the Varieties of American Progressivism" (Ph.D. dissertation, Vanderbilt University, 1969); Evan Marcus Anders, "Bosses under Siege: The Politics of South Texas during the Progressive Era" (Ph.D. dissertation, University of Texas, Austin, 1978); and J. Michael Shahan, "Reform and Politics in Tennessee, 1906-1914" (Ph.D. dissertation, Vanderbilt University, 1981).

<sup>57</sup> For a recent, thoughtful examination of the progressive impulse, including regulatory campaigns in the South, see Richard L. McCormick, "The Discovery that Business Corrupts Politics: A Reappraisal of the Origins of Progressivism," *AHR*, 86 (1981): 247-74, esp. 261-62.

flected the growth of social services in the South during the previous decade, the organization of several state conferences on charities and correction, and a heightened awareness among Southern progressives of social reforms in other parts of the country. The congress stimulated interest in attacking social problems, including racial injustice, encouraged the establishment of other social welfare groups, and exerted some influence in the enactment of social reform legislation in the individual states.<sup>58</sup>

The nationalization of reform after 1912 was apparent in two of the Southern progressives' most vigorous regulatory movements: the campaign to control railroads and the prohibition crusade. The barriers to effective regulation of railroads by state legislatures and commissions, including the inherent problem of dealing with interstate companies and the restraints imposed by federal court decisions and Interstate Commerce Commission rulings, brought a shift from reliance on state regulation to national control. Meanwhile, the vexatious task of enforcing state and local prohibition in dry areas, not to mention the defeat of the antiliquor cause in Florida, Texas, and Alabama, led many Southern reformers to fall in line with the American Anti-Saloon League's call for national prohibition. The tendency to look for national remedies was also evident in numerous other progressive campaigns: in the child labor reform movement; in the demand by the Farmers' Union and other agricultural pressure groups for federal regulations, credit facilities, and farm demonstration programs; and in the willingness of organizations such as the Southern Commercial Congress to turn to Washington for assistance in dealing with problems like flood control. This transition from state to federal action was hastened by the activities of such organizations as the National Child Labor Committee, the American Anti-Saloon League, the Commission on Country Life, and the National Association of Charities and Correction.

The South was slower in lending support to the National American Woman Suffrage Association's efforts to secure the enfranchisement of women through congressional action and a constitutional amendment. Although tiny woman suffrage groups were formed in most Southern states in the 1880s and 1890s, they were feeble and many of them soon disappeared. There was little concerted activity in behalf of the votes-for-women cause before 1910. Nevertheless, the intensity of "the woman's movement" in the region had increased, and feminine involvement in the campaign to regulate child labor, the educational crusade, and the prohibition movement, not to mention numerous community projects, indicates the distance the reformers had come since joining the missionary societies and the women's clubs. As time passed this feminist reformism tended to find greater focus in the drive for the suffrage. During the five or six years before the United States entered World War I, Southern suffragists developed state-

<sup>58</sup> For the origins and activities of the Southern Sociological Congress, see E. Charles Chatfield, "The Southern Sociological Congress: Organization of Uplift," *Tennessee Historical Quarterly*, 19 (1960): 328-47, and "The Southern Sociological Congress: Rationale of Uplift," *ibid.*, 20 (1961): 51-64; and James E. McCulloch, ed., *The Call of the New South: Addresses Delivered at the Southern Sociological Congress, Nashville, Tennessee, May 7 to 10, 1912* (Nashville, Tenn., 1912).

wide associations, organized hundreds of local societies, and launched campaigns to influence legislators and the public. Their objective was to win the vote with favorable decisions in the statehouses, but they encountered formidable resistance. As their strength grew and their frustrations mounted in the wake of setbacks at the state level, Southern suffragists began to work more actively for a federal amendment.<sup>59</sup>

Meanwhile, Southern politics showed signs of increasing involvement in the debate over national issues and elections. In 1910 the Democrats won control of the House of Representatives, and, when the House organized for business in 1911, the seniority rule brought a group of Southerners to prominence as chairmen of important committees. Southern interest in national politics rose to new heights with the nomination and election of Woodrow Wilson as the nation's twenty-eighth president. Southerners such as Walter Hines Page and William G. McAdoo were prominent in promoting Wilson's candidacy. The movement for his nomination was closely identified with progressive politics in the South, and in a broader sense its impact upon the diffused currents of social reform in the region was profound.<sup>60</sup> In short, the Wilson movement marked an important stage in the course of Southern progressivism. Furthermore, during the Wilson years the region took a long step in the direction of a more positive role in national politics.

Southern congressmen, who dominated the standing committees in both houses of Congress, strongly supported the principal legislation of Wilson's New Freedom.<sup>61</sup> Nevertheless, there was strong Southern opposition to many of the more advanced progressive measures considered during Wilson's first term. For example, Southerners were in the vanguard of the successful opposition in 1914 and 1915 to a constitutional amendment to enfranchise women. The Southern lawmakers were much more inclined to support certain measures of social con-

<sup>59</sup> A. Elizabeth Taylor has written a valuable monograph and a series of informative articles on the woman suffrage movement in the various Southern states; see, for example, Taylor, *The Woman Suffrage Movement in Tennessee* (New York, 1957), "The Woman Suffrage Movement in Texas," *JSH*, 17 (1951): 194-215, "The Woman Suffrage Movement in Arkansas," *Arkansas Historical Quarterly*, 15 (1956): 17-52, "Revival and Development of the Woman Suffrage Movement in Georgia," *Georgia Historical Quarterly*, 42 (1958): 339-54, "The Woman Suffrage Movement in Mississippi, 1890-1920," *Journal of Mississippi History*, 30 (1968): 1-34, and "South Carolina and the Enfranchisement of Women: The Later Years," *South Carolina Historical Magazine*, 80 (1979): 298-310. Also see Aileen S. Kraditor, *The Ideas of the Woman Suffrage Movement, 1890-1920* (New York, 1965); Anne Firor Scott, *The Southern Lady: From Pedestal to Politics, 1830-1930* (Chicago, 1970), 165-84; Paul E. Fuller, *Laura Clay and the Woman's Rights Movement* (Lexington, Ky., 1975); Lee N. Allen, "The Woman Suffrage Movement in Alabama, 1910-1920," *Alabama Review*, 11 (1958): 83-99; Kenneth R. Johnson, "The Woman Suffrage Movement in Florida" (Ph.D. dissertation, Florida State University, 1966), and "Kate Gordon and the Woman-Suffrage Movement in the South," *JSH*, 38 (1972): 365-92; and Louise Boyd James, "The Woman Suffrage Issue in the Oklahoma Constitutional Convention," *Chronicles of Oklahoma*, 56 (1979): 379-92.

<sup>60</sup> Link, "The Progressive Movement in the South," 180-81.

<sup>61</sup> Arthur S. Link, *Woodrow Wilson and the Progressive Era, 1910-1917* (New York, 1954), 25-80, and "The South and the 'New Freedom': An Interpretation," *American Scholar*, 20 (1951): 314-24; Saloutos, *Farmer Movements in the South*, 213-35; Anne Firor Scott, "The Southern Progressives in National Politics, 1906-1916" (Ph.D. dissertation, Radcliffe College, 1957), 164-82; John W. Davidson, "The Response of the South to Woodrow Wilson's New Freedom, 1912-1914" (Ph.D. dissertation, Yale University, 1953); Tindall, *The Emergence of the New South*, 10-17; and Dewey W. Grantham, "Southern Congressional Leaders and the New Freedom, 1913-1917," *JSH*, 13 (1947): 439-59. For an interpretation that minimizes Southern reformism and the role of congressmen from the South, see Richard M. Abrams, "Woodrow Wilson and the Southern Congressmen, 1913-1916," *JSH*, 22 (1956): 417-37.



trol, such as national prohibition, immigration restriction, and especially Negro proscription in Washington and the federal service.<sup>62</sup> In general, however, public opinion in the South and the section's congressmen found much to applaud in Wilson's New Freedom. Meanwhile, the progressive spirit, stimulated by the Wilsonian example, became manifest in new reform campaigns and in new advances in state services and administration in the South.

In some respects the First World War weakened and redirected the currents of social reform in the United States, particularly after the nation entered the conflict in April 1917. Yet the war also created new opportunities for social planning and even nourished the progressive belief that "by altering the environment it was possible to reconstruct society."<sup>63</sup> The war had a momentous effect on Southern society. It contributed to the region's prosperity, brought an expansion in the functions of government, encouraged civic cooperation, enhanced the role of voluntary groups, and opened new avenues of social control, efficiency, and social justice. Preoccupation with the war effort was evident in virtually every aspect of Southern life. The region's congressmen, entrenched in powerful committee positions, provided indispensable legislative assistance in the enactment of the administration's wartime program.<sup>64</sup> The mobilization of community resources for military purposes had important consequences for the development of new social services in the Southern states. As Hastings H. Hart of the Russell Sage Foundation wrote during the war, in reporting on the social agencies and institutions of South Carolina, that state had an opportunity "so to direct its war work in production and food conservation, in guarding the health and morals of the soldier, in the care of invalid, crippled, and insane soldiers, and in the training of youth for patriotic service as greatly to improve the quality and efficiency of the social work of the State for all future time."<sup>65</sup>

Some Southern states took advantage of the wartime atmosphere to adopt new social welfare legislation. North Carolina, for example, reorganized its public welfare program during the years 1917-19.<sup>66</sup> The war also brought to fruition three reforms for which Southern progressives had long struggled. One of these was national prohibition, an achievement that owed much to Southern congressmen, state legislators, and public opinion. Another was the adoption of the Nineteenth Amendment enfranchising women. During the war the woman suffrage movement had emerged as a serious reform cause in the South. Although a majority of the Southern congressmen voted against the amendment

<sup>62</sup> Scott, "The Southern Progressives in National Politics," 186-92, 195-99; Tindall, *The Emergence of the New South*, 16, 220-23; Link, *Woodrow Wilson and the Progressive Era*, 63-66; and Morton Sosna, "The South in the Saddle: Racial Politics during the Wilson Years," *Wisconsin Magazine of History*, 54 (1970): 30-49.

<sup>63</sup> Allen F. Davis, "Welfare, Reform, and World War I," *American Quarterly*, 19 (1967): 517.

<sup>64</sup> Richard L. Watson, Jr., "A Testing Time for Southern Congressional Leadership: The War Crisis of 1917-1918," *JSH*, 44 (1978): 3-40, and "Principle, Party, and Constituency: The North Carolina Congressional Delegation, 1917-1919," *North Carolina Historical Review*, 56 (1979): 298-323.

<sup>65</sup> Hart, *The War Program of the State of South Carolina: A Report Prepared at the Request of Governor Richard I. Manning, the State Council of Defense, and the State Board of Charities and Corrections* (New York, 1918), 14.

<sup>66</sup> Howard W. Odum, "The North Carolina Plan," in Odum and Willard, *Systems of Public Welfare*, 172-73; and Andrew Dobelstein, "The Effects of the Reform Movement on Relief Administration in North Carolina: The Contributions of Alexander Worth McAlister," *South Atlantic Quarterly*, 75 (1976): 245-57.

and it encountered powerful opposition in the section's state legislatures, it was supported by an increasing number of Southerners. The third reform occurred in the search for progress in race relations. The attention given this perennial question by the Southern Sociological Congress and the efforts of people like Willis D. Weatherford, James H. Dillard, and Will W. Alexander helped pave the way for tangible action. Even so, the exigencies and opportunities of the war and its aftermath provided the immediate impetus for the establishment of the Commission on Interracial Cooperation in 1919.

THE END OF THE WAR and the collapse of the Wilson administration in 1919 and 1920 coincided with and contributed to the disintegration of Southern progressivism. By that time the regional focus of the reformers had been disrupted, and they were no longer united by a common program of social reforms and values. They had also lost much of their élan and optimism as progressives. Social change, accelerated by the war, created new tensions, and the postwar years were exacerbated by sharp conflicts among Southerners and between the South and other sections. The intersectional struggle for control of the Democratic party eroded the progressive balance in the South and added to the saliency of cultural issues like prohibition. In other words, the harmony that had held the major components of Southern progressivism in balance no longer existed. The earlier successes of Southern progressivism may also have contributed to its loss of vitality in the 1920s. There were signs that the progressives had about reached the limits of their concept of reform. A new generation of leaders was coming to the fore. Furthermore, many professional groups were making the transition from "the missionary era to one of institutionalization and professionalism." Caught up in the struggle to develop effective agencies and services in their particular fields and in their professional growth and recognition, they found the old kinds of social reform increasingly irrelevant and unsatisfying. Southern social workers, for example, like their counterparts in the rest of the country, now seemed more concerned with "procedure and the adjustment of the individual to his environment" than in transforming the social environment in which the individual lived.<sup>67</sup>

Nevertheless, progressivism in the South did not disappear in the 1920s. Instead, as George B. Tindall has shown, it was "transformed through greater emphasis upon certain of its tendencies and the distortion of others."<sup>68</sup> One of the surviving strains of Southern progressivism expressed itself in the zealous campaign to enforce prohibition, in the fundamentalist movement, and in other efforts to protect moral standards and traditional cultural values. Another was what Tindall has termed "business progressivism"—the intensified emphasis on "good government," administrative efficiency, and more adequate public serv-

<sup>67</sup> Tindall, *The Emergence of the New South*, 254; and Clarke A. Chambers, *Seedtime of Reform: American Social Service and Social Action, 1918-1933* (Minneapolis, 1963), 93.

<sup>68</sup> Tindall, *The Emergence of the New South*, 219.

ices.<sup>69</sup> Racial attitudes and practices provide a third example. Southern reformers, building upon their modest efforts of earlier years to soften the harsh rigidities of race relations and to continue the limited interracial cooperation of the war period, undertook a major reform initiative in this period. The creation of the Commission on Interracial Cooperation represented an organized endeavor for harmonious race relations and, unlike earlier white reformism in this area, pointed to a concrete and realistic mode of action. But at the same time the commission was carefully restricted by the bounds of white orthodoxy.

In 1926 Edwin Mims, a participant in many of the progressive campaigns of the past quarter-century, published *The Advancing South*. Mims's book was a spirited and hopeful account of "progress and reaction," of what he described as the "war of liberation" and the long struggle "for emancipation from outworn traditions" in the South.<sup>70</sup> Although Mims was not a representative progressive, he captured the idealism of the reformers, their hopefulness, their moderation, and their middle-class values. He instinctively understood their almost compulsive reliance on the South as an idea and on the region as an arena for reform, and he appreciated their ardent desire to reconcile tradition with progress, to mediate between classes, races, and sections, to bring stability, harmony, and enlightenment to Southern society. Yet *The Advancing South* was curiously limited as a characterization of Southern progressivism. The interpretation gave short shrift to populism, the antitrust movement, and political insurgency and factionalism in the Democratic party. Nor did it make sufficient allowance for the role of prohibition and other campaigns to enforce morality in the early twentieth-century South. Mims was correct in asserting that organized action had brought progressive advances to the South during this era: in economic development, social welfare programs, more adequate public services, and the modernization of institutions. But his portrayal scarcely did justice to the comprehensiveness and complexity of Southern progressivism.

Progressivism in the South had a marked effect on the region's politics, social action, and social thought during the early twentieth century. This was true in large part because Southern progressives brought together in tolerable equipoise a number of assumptions and ideas about the nature and development of the South and elicited support from diverse social elements, including the section's civic-commercial elites and upwardly mobile urban groups. The progressives, as Tindall has argued, effected a synthesis of the antithetical approaches of the Bourbons and Populists.<sup>71</sup> There was room in this complex of ideas for material progress, efficiency, ethical standards, social order, a more vigorous regulatory state, social justice, public services, and especially the vision of a revitalized regional community. In the early twentieth-century setting the progressives were

<sup>69</sup> Tindall, "Business Progressivism: Southern Politics in the Twenties," *South Atlantic Quarterly*, 62 (1963): 92-106, and *The Emergence of the New South*, 254-84. Also see Charles Orson Cook, "Boosterism and Babbitry: Charles Hillman Brough and the 'Selling' of Arkansas," *Arkansas Historical Quarterly*, 37 (1978): 74-83.

<sup>70</sup> Mims, *The Advancing South: Stories of Progress and Reaction* (Garden City, N.Y., 1926), vii-viii. Also see Leah Marie Park, "Edwin Mims and *The Advancing South* (1894-1926): A Study of a Southern Liberal" (M.A. thesis, Vanderbilt University, 1964).

<sup>71</sup> Tindall, *The Persistent Tradition in New South Politics* (Baton Rouge, 1975).

able to function both as agents of modernization and as guardians of Southern tradition.

The legacy of Southern progressivism was also important. The social values, approach, and style of the progressives created an enduring design in the modern South. Thus tradition and modernization, both encompassed in Southern progressivism, strongly influenced the section's political culture during the 1920s. One aspect of Southern progressivism looked toward intersectional accommodation, national involvement, and national solutions to regional problems, and those Southerners who represented this approach welcomed the New Deal and later liberal administrations in Washington. Other Southerners were upset and increasingly alienated by this brand of national social action. Keenly aware of the South's pressing social problems and the many regional barriers to progress as well as the fragility of their own strength and resources, Southern liberals were, in Gunnar Myrdal's words, "inclined to stress the need for patience and to exalt the cautious approach, the slow change, the organic nature of social growth."<sup>72</sup> While the early twentieth-century progressives were cautious reformers, they were the first Southerners to make a concerted effort to cope with social problems growing out of the new industrial and urbanized system. The liberal tradition they did so much to establish survived in the antitrust and consumer protection politics of men like Hugo L. Black, Claude Pepper, and Estes Kefauver.<sup>73</sup> But, in general, the South's recent political leaders, including most liberals, continued to emphasize the essential role of economic development in the rehabilitation of the region. Like the progressives, moreover, they also continued to emphasize the need to broaden and unite the middle class in order to deal with contemporary social problems.

THE COMPLEXITY OF SOUTHERN PROGRESSIVISM and the richness of its historiography present its interpreters with both a challenge and an opportunity. Clearly the time is ripe for a comprehensive historical synthesis of reform in the South during the Progressive era. The focus of such a work, one would think, should be the distinctive patterns of reform in the Southern states and localities between the 1890s and the 1920s, and the progressive organizations and campaigns that assumed a regional character. Most of the individual reform movements need more intensive research and historical explanation. Even those reform causes that have been investigated most thoroughly—education, child labor, prohibition, the prison system, agricultural rehabilitation, and the like—have seldom been interpreted in the larger context of regional and national progressivism. Nor have the sources of support for the various movements been illuminated. The regional and national dimensions of Southern progressivism after 1912 could be more fully revealed with concentrated research in three

<sup>72</sup> Myrdal, with Richard Sterner and Arnold Rose, *An American Dilemma: The Negro Problem and Modern Democracy*, 1 (New York, 1944): 470.

<sup>73</sup> See, for example, Richard E. McFadyen, "Estes Kefauver and the Tradition of Southern Progressivism," *Tennessee Historical Quarterly*, 37 (1978): 430–43.

areas, each of which deserves separate treatment. One of these is Southern progressivism and the Wilson administration during the years 1913–17. Another is social reform in the South during the First World War. The third is the denouement of Southern progressivism and the nature of social reform in the 1920s.

There are numerous studies of progressivism in the South at the state level, many of them as yet unpublished. These works are valuable for their descriptions of progressive organizations, campaigns, and legislative struggles; their interpretive qualities are less impressive. The conceptual, analytical, and interpretive strengths of Raymond H. Pulley's *Old Virginia Restored* (1968) and Sheldon Hackney's *Populism to Progressivism in Alabama* (1969) place those volumes in a class by themselves. Similar studies of other Southern states would be welcome additions to the scholarly literature. Most of the completed monographs neglect the organizational basis and the role of functional groups in reform movements. Concentration on the state level should permit scholars to do much more than has been done in identifying the constituent elements and rank-and-file supporters of progressivism in the South. There are still only a few books and dissertations on politics and social reform in individual Southern states during the Wilson years, and even fewer for the 1920s.<sup>74</sup> Although biography has been a favorite mode of historical inquiry into state and local progressivism, many significant figures have not yet received treatment, including Theodore G. Bilbo, Thomas M. Campbell, James Cannon, Jr., Walter Clark, Claude Kitchin, and Luke Lea.<sup>75</sup>

Urban history and other local studies of Southern places during the Progressive era constitute one of the most promising approaches to a fuller understanding of progressivism in the South. A series of monographs and dissertations on Atlanta suggests what can be done for other Southern cities.<sup>76</sup> It is increasingly apparent from ongoing research that the region's cities were actively involved in the quest for greater order, efficiency, and well-being in American urban life. Much more research needs to be done on the nature of this municipal reform in the South.<sup>77</sup> Carl V. Harris's analysis, *Political Power in Birmingham* (1977), sug-

<sup>74</sup> For one Southern state, see David D. Lee, *Tennessee in Turmoil: Politics in the Volunteer State, 1920–1932* (Memphis, 1979); and Joseph T. Macpherson, "Democratic Progressivism in Tennessee: The Administrations of Governor Austin Peay, 1923–1927" (Ph.D. dissertation, Vanderbilt University, 1969). For research on another Southern state, see Katherine D. Cann, "South Carolina in the 1920's" (Ph.D. dissertation, University of South Carolina, in progress).

<sup>75</sup> Among numerous unpublished dissertations, see Henry Clifton Ferrell, Jr., "Claude A. Swanson of Virginia" (University of Virginia, 1964); Doyle Willard Boggs, Jr., "John Patrick Grace and the Politics of Reform in South Carolina, 1900–1931" (University of South Carolina, 1977); Stephen Harry Kerber, "Park Trammell of Florida: A Political Biography" (University of Florida, 1979); Charles O. Cook, "Charles Hillman Brough: Arkansas Progressive" (University of Houston, 1980); and Charlotte Jean Shelton, "William Atkinson Jones, 1849–1918: Independent Democracy in Gilded Age Virginia" (University of Virginia, 1980).

<sup>76</sup> Eugene J. Watts, *The Social Bases of City Politics: Atlanta, 1865–1903* (Westport, Conn., 1978); Howard L. Preston, *Automobile Age Atlanta: The Making of a Southern Metropolis, 1900–1935* (Athens, Ga., 1979); Thomas M. Deaton, "Atlanta during the Progressive Era" (Ph.D. dissertation, University of Georgia, 1969); Elizabeth Anne Mack Lyon, "Business Buildings in Atlanta: A Study in Urban Growth and Form" (Ph.D. dissertation, Emory University, 1971); Willie Miller Bolden, "The Political Structure of Charter Revision Movements in Atlanta during the Progressive Era" (Ph.D. dissertation, Emory University, 1978); and Martha T. Nesbitt, "The Social Gospel Movement in Atlanta, 1900–1920" (Ph.D. dissertation, Georgia State University, 1978).

<sup>77</sup> For examples of urban histories that throw light on Southern progressivism, see John J. Duffy, "Charleston Politics in the Progressive Era" (Ph.D. dissertation, University of South Carolina, 1963); Richard H. L.



gests one direction for the study of urban reform in this period—the question of what classes and interest groups wielded power and what influence particular groups exerted in the introduction of structural changes, improved public services, and social welfare programs in the urban South. Scholars are beginning to study the economic and social conditions of workers, blacks, and other urban dwellers without much power.<sup>78</sup> But the role of these elements in municipal reform in the South and the way in which they were affected by “progressive” measures deserve more scholarly inquiry. John Dittmer’s *Black Georgia in the Progressive Era* (1977) and Lester C. Lamon’s *Black Tennesseans, 1900–1930* (1977) provide vivid illustrations, for instance, of social change and reform efforts in the black community during this period.

Some of the themes in Southern progressivism have received more scholarly attention than others. In general, the reform movements that sought to regulate business practices and apply certain social controls have been examined most thoroughly. Yet even in this category the railroad regulatory campaign as a regional movement awaits its historian. Another significant area of neglect is the movement for social justice. A major reason for this neglect, no doubt, is that social justice was less important in the South than in other regions. But, even so, noteworthy steps were taken to ameliorate social distress—through private beneficence, organized charity, church missions, settlement houses, and public welfare. Social justice pioneers, many of whom were women, were at work in every Southern state and most of the region’s cities by 1910. As meliorative programs evolved and were slowly institutionalized, social work as a profession gradually developed in the South. None of these developments has been adequately studied by historians.<sup>79</sup>

Social efficiency offers another promising topic for further research and analy-

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German, “The Queen City of the Savannah: Augusta, Georgia, during the Urban Progressive Era, 1890–1917” (Ph.D. dissertation, University of Florida, 1971); Samuel M. Kipp III, “Urban Growth and Social Change in the South, 1870–1920: Greensboro, North Carolina as a Case Study” (Ph.D. dissertation, Princeton University, 1974); and James R. McGovern, *The Emergence of a City in the Modern South: Pensacola, 1900–1945* (Pensacola, 1976). For the 1920s, see Blaine A. Brownell, *The Urban Ethos in the South, 1920–1930* (Baton Rouge, 1975).

<sup>78</sup> See, for example, George C. Wright, “Blacks in Louisville, Kentucky, 1890–1930” (Ph.D. dissertation, Duke University, 1977); Richard A. Straw, “This Is Not a Strike, It Is Simply a Revolution: Birmingham Miners Struggle for Power, 1894–1908” (Ph.D. dissertation, University of Missouri, Columbia, 1980); Dolores E. Janiewski, “From Field to Factory: Race, Class, Sex, and the Woman Worker in Durham, 1880–1940” (Ph.D. dissertation, Duke University, 1980); and Elsa B. Brown, “Richmond Black Community, 1890–1930” (Ph.D. dissertation, Kent State University, in progress).

<sup>79</sup> For evidence of recent research in these areas, see William J. Breen, “Southern Women in the War: The North Carolina Woman’s Committee, 1917–1919,” *North Carolina Historical Review*, 55 (1978): 251–83, and “Black Women and the Great War: Mobilization and Reform in the South,” *JSH*, 44 (1978): 421–40; Darlene Rebecca Roth, “Matronage: Patterns in Women’s Organizations, Atlanta, Georgia, 1890–1940” (Ph.D. dissertation, George Washington University, 1978); Albert J. Ettling, Jr., “The Germ of Laziness: The Rockefeller Sanitary Commission in the Southern States, 1909–1914” (Ph.D. dissertation, Harvard University, 1978); Melba Porter Hay, “Madeline McDowell Breckenridge: Kentucky Suffragist and Progressive Reformer” (Ph.D. dissertation, University of Kentucky, 1980); John William Klein, “The Role and Impact of Rockefeller Philanthropy during the Progressive Era” (Ph.D. dissertation, Fordham University, 1980); Samuel C. Shepherd, Jr., “Churches at Work: Richmond, Virginia, White Protestant Leaders and Social Change in a Southern City, 1900–1929” (Ph.D. dissertation, University of Wisconsin, Madison, 1980); and Marlene H. Rickard, “Case Study of Welfare Capitalism: The Welfare Programs of the Tennessee Coal, Iron, and Railroad Company (U.S. Steel) of Birmingham, Alabama” (Ph.D. dissertation, University of Alabama, in progress).

sis. It was a principal concern of the progressives in virtually all of their reform undertakings—from the improvement of agriculture to the modernization of urban institutions—and an important factor in the growth of public services during the first two decades of this century. A variety of public services came of age in the Southern states and cities during the 1920s. But just how state and municipal support of education, highways, public health, reclamation and conservation, and various welfare programs evolved during the Progressive years is not yet clear.<sup>80</sup>

The research needs identified in this discussion have become salient largely because of the extensive body of scholarly writing that now exists on Southern progressivism. This historiography has contributed to the historical reconstruction of the American South and of the Progressive movement in the United States and, within these broad contexts, has extended and deepened our understanding of the region and the movement. But much more work remains to be done.

<sup>80</sup> See, for example, Nelson Manfred Blake, *Land into Water—Water into Land: A History of Water Management in Florida* (Tallahassee, 1980), 88–128; Frank Bedingfield Vinson, “Conservation and the South, 1890–1920” (Ph.D. dissertation, University of Georgia, 1971); and Craig Austin Michalik, “The Southern State University during the Progressive Era” (Ph.D. dissertation, University of Arkansas, 1978).

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## Reviews of Books

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### GENERAL

JAMES H. BILLINGTON. *Fire in the Minds of Men: Origins of the Revolutionary Faith*. New York: Basic Books. 1980. Pp. viii, 677. \$25.00.

"Les extrêmes se touchent." This chapter heading of Louis-Sebastian Mercier's *Tableaux de Paris* anticipates, says James H. Billington, "the affinity and conscious borrowings between the extremes of Right and Left" (p. 117). Whether intentionally or not, Billington's *Fire in the Minds of Men* illustrates that affinity. It keeps the extremes in touch, and in tension, from start to finish. Fascinated by the filiation of apparently antithetical revolutionaries such as Auguste Blanqui and Karl Marx, drawn by occult revolutionary symbols, captivated by exotic revolutionary beliefs, Billington's antirevolutionary romanticism leads him from the Palais Royal's corrupt cafés in 1789 to Berlin's insurrectionary streets in 1919. Over that period, Billington traces the construction, elaboration, and application in Europe of what he calls the "revolutionary tradition": the belief in an unfinished revolution, in the authority of nature against tradition, and in secret, hierarchical organization as the instrument of revolutionary transformation. Drawing on vast reading in French, German, Russian, and a half-dozen other European languages, he spins an immense, fragile web of connections among distinct ideas, practices, events, and, especially, thinkers.

Those thinkers! *Fire in the Minds of Men* stages a fantastic parade of unfamiliar revolutionaries, and of familiar revolutionaries in unfamiliar costumes. Before our eyes "father" Barthélemy Prosper Enfantin leads a search for the female Messiah in Egypt, the West Indian mulatto Thomas Urbain becomes a Saint-Simonian Muslim prophet in Algeria, and V. I. Lenin himself appears garbed as a wild evangelist: "Like the nine-headed beast in the Book of Revelation, the 'ravening beasts of finance capitalism' were for Lenin a sign that the end of history was near. The millennium, of course, was the coming classless society. And just as the antichrist pre-

ceded the true Christ in Christian eschatology, so the imperialist war had concentrated power and mobilized the masses in ways that would make a proletarian take-over easier" (p. 466). Spirited away is Lenin the tactical virtuoso, occulted Lenin the zealous administrator. Instead Billington shows us a Lenin "baptized" into the revolutionary faith, "rebaptized" a proletarian, nurturing the spark—the *Iskra*—which would become a consuming flame. If the portrayal is a travesty, at least it is a colorful travesty.

The book's chief arguments likewise strut in motley. When being sober and programmatic, Billington stresses the conditions for emergence of a truly revolutionary movement: the absence of a previous break with medieval religious authority, the absence of an organized, tolerated political opposition, a weak bourgeoisie. Few of the text's 509 pages and 143 pages of notes, however, concern those arguments. Nor does any summary or conclusion review them at the book's close; after an eloquent evocation of Rosa Luxemburg as the patron saint of Polish resistance to Soviet hegemony, Billington moves to a brief epilogue on revolutionary ideas outside Europe.

Billington devotes much more of the book to a second manifest line of argument: a model of the origin and development of revolutionary thought in the West. The revolutionary tradition, thinks Billington, sprang from German illuminism rather than French Enlightenment thought, spread and changed mainly through the efforts of literary intellectuals, passed from an early stage of republican nationalism (patterned implicitly on the static Masonic lodge) to a struggle between that complex of beliefs and social internationalism (patterned implicitly on fuel-powered machines). At one point, Proudhon exemplified the first stage, Marx the second. Lenin and the Bolsheviks then finally merged the Marxist tradition with some elements of the Proudhonist: "The Russian Revolution of 1917 was the classic revolution for social equality. But the Soviet leaders adopted as well the language of liberal and national revolutionaries—and debased the en-

tire revolutionary vocabulary by using it to rationalize imperial despotism" (p. 10).

A third line of argument peeps out from its various disguises throughout the book but only bursts into the open in the final sentences: "When a Cuban national revolution came into conflict with the imperial power of the first nation to be born in revolution, the United States," writes Billington, "it attracted considerable sympathy—but more among well-fed young students in the overdeveloped West than among the hungry in the underdeveloped world. Utopia for many intellectuals had simply returned to a tropical island in the New World—which is where the intellectuals of early modern Europe had always imagined it might be" (p. 509). Nasty, but thought-provoking.

Similarly, Billington uses asides and illustrations to state other recurrent arguments, most of them debunking: that genuine workers rarely subscribed to the revolutionary vision; that the far left not only resembled the extreme right but also achieved a number of its victories with the unwitting assistance of the extreme right; that revolutionary beliefs constituted a secular religion; that Marx was merely one among a crowd of similar revolutionaries; that Marx borrowed heavily from Blanqui; that Mussolini and Lenin had much in common; and so on. Although some of these arguments are more seriously laid out and worthier of attention than others, together they amount to a sustained, passionate, romantic attack on revolutionaries' claims to benefit from a distinctive, viable intellectual tradition.

To establish the book's first block of arguments—the importance of Protestantism as a barrier to genuinely revolutionary movements, and so on—would require systematic comparisons among European regions and a consideration of apparent exceptions such as the American Revolution or Chartism. To validate the model of revolutionary thought's origins and development would require something more than the assertions of correspondence and connection by which the argument proceeds; it would require much more ruling out of competing accounts. To establish its underground arguments would require a reasoned comparison of revolutionary theory with its alternatives. None of this happens. From the book's opening pages, Billington proposes an exploration, not a rigorous survey.

Sometimes the explorer stumbles. In the case of France, for example, he confuses 1789's *Grande Peur* with its peasant revolts (p. 21), claims that in 1848 the "ferment" that produced the revolution "was clearly dominated by intellectuals" (p. 208), and calls the Commune of 1871 "narrowly Parisian" (p. 347) despite the early Communes of Toulouse, Marseilles, and other provincial capitals. Such secondary errors are, I suppose, not much price to pay for passages like the one (pp. 58–72) in which Bill-

ington portrays Strasbourg as at once the port of entry into France for German prerevolutionary thought, the origin of the prophet of revolution Saint-Just, and the birthplace of the great revolutionary song "La Marseillaise." Billington's treatment of the revolutionary, then conservative, place of opera; his irreverent and fanciful parallels (for example, the "revolutionary variety show" of Lenin-grad's Cirque Moderne in 1917 and the incessant rallying of Paris's Cirque du Palais Royal in 1789 (p. 468); his deployment of minor but symbolically charged facts (such as Benito Mussolini's having been named for Benito Juarez); and similar devices make the many pages of *Fire in the Minds of Men* move rapidly, if jerkily, like a mistuned internal combustion engine. That internal combustion engine, a machine driven by incessant fire, epitomizes James Billington's late romantic, antirevolutionary review of revolutionary ideas.

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A. RUPERT HALL. *Philosophers at War: The Quarrel between Newton and Leibniz*. New York: Cambridge University Press. 1980. Pp. xiii, 338. \$24.95.

The most famous and most acrimonious dispute over scientific priority arose over the claims of Newton and of Leibniz to have discovered the calculus. Thanks to historical research, we now know that Newton was in fact the "first discoverer," since he developed the calculus during the years from 1664 to 1666, whereas Leibniz made his discovery only nine years later in 1675. Also, there is no question of the fact that Leibniz has absolute priority of publication, since he revealed his system in print in 1684, whereas Newton did not publish any complete mathematical tract until twenty years later, when he allowed a work on the "quadrature" of curves to be printed as an appendix to his *Opticks* (1704). The Newtonian claim that Leibniz had learned about the calculus from Newton was largely based on the fact that in 1676 Newton had written Leibniz two letters on technical mathematical topics; furthermore, while in London Leibniz studied and took notes on some of Newton's manuscripts that were then being circulated among mathematicians in England. The fact that Leibniz's notes survive enable us to confirm the judgment, based on the contents of Newton's letters to Leibniz, that what Newton conveyed to Leibniz was primarily information concerning infinite series. The letters contain only indirect reference to the calculus. Accordingly, it is possible today to make the simple unqualified judgment that Newton and Leibniz were independent discoverers (or "inventors") of the

calculus, the major mathematical breakthrough of the seventeenth century, and for most people the starting point of modern mathematical analysis.

In the volume under review, A. Rupert Hall describes the stages of relations between Newton and Leibniz from the early epistolary exchanges to the beginning of the quarrel over priority that opened in 1710, when the Newtonian mathematician John Keill published a paper suggesting that Leibniz had plagiarized his ideas about the calculus from Newton. When Leibniz wrote to the Royal Society to demand an apology for this stain on his character, the president of the society, Isaac Newton himself, chose an "international" committee to study the matter. We now know that Newton wrote the drafts of the committee's report, published in 1712-13, and wrote and published an anonymous review of the report in the Royal Society's journal, *The Philosophical Transactions*. He then wrote an anonymous preface, "To the Reader," for a reprint of the report by the supposedly impartial and international committee. The reprint also contained a Latin version of the review written by Newton. During the years of conflict neither Newton nor Leibniz acted in what we would call a straightforward or exemplary manner, nor did their followers. All of the various maneuvers in the conflict are described clearly in Hall's volume, which is concerned primarily with the character and activities of the protagonists. One of the major contributions that Hall has made in the volume is to give us information concerning the secondary characters in England and on the Continent who became involved in this struggle.

Among the merits of Hall's volume is the fact that he relates the quarrel concerning the calculus and its invention to the ongoing debate between Continental mathematicians, physicists, and astronomers and their British counterparts over the concepts of force and universal gravitation—the latter having been particularly attacked by Leibniz as a return to the rejected "occult" qualities and forces. It is one of the curiosities of history that the Continental algorithm for the calculus should have triumphed at the same time that the Newtonian natural philosophy based on forces and universal gravitation became universally adopted. Hall has largely eliminated the mathematical technicalities from his volume, so that the stages of the controversy can be more available to the general reader. The technical mathematical materials concerning this subject are to be contained in the forthcoming final volume of D. T. Whiteside's magisterial edition of Newton's *Mathematical Papers*. Many readers, however, while being grateful to Hall for his presentation of the main stages of this acrimonious debate, will nevertheless wish that he had not so completely eliminated the mathematical content, since without it the volume loses much of

its point. The general reader who is ignorant of the mathematical significance may find it difficult to understand why the controversy was so heated.

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ALLEN W. WOOD. *Karl Marx*. (Arguments of the Philosophers.) Boston: Routledge and Kegan Paul. 1981. Pp. xviii, 282. \$25.00.

This volume is part of a series entitled "Arguments of the Philosophers" and, accordingly, focuses on the "philosophy" of Karl Marx. One may doubt whether Marx is best classified as a philosopher and placed together with Descartes, Berkeley, Russell, Wittgenstein, and the other philosopher-subjects of this series, but this is to cavil. Marx's social and economic theorizing clearly has a philosophical side to it, and the themes treated by Allen W. Wood are important ones for understanding Marx's world-view.

The five parts of Wood's book concern, respectively, alienation, historical materialism, morality, philosophical materialism, and the dialectical method. The concept of alienation has enjoyed wide appeal in our time, and Wood explains what it meant for Marx and how it figures in his critique of capitalism. He contends that while the concept was supposed to perform an explanatory function in Marx's early writings, his later use of it was only descriptive. Throughout his life, however, Marx shared with Aristotle a view of the fulfilling life as one that exercises and develops our essential human capacities. Unlike many commentators, Wood is not overly impressed by the philosophical insight of Marx's early manuscripts, and he readily concedes that alienation was not for Marx the only, or even the most important, evil of capitalism.

Wood's explication of historical materialism (which is basically an empirical, rather than a philosophical theory) is accurate, and his discussion of such issues as determinism, ideology, and the form of Marx's materialist explanations is useful. Wood has less to say about Marx's materialist naturalism, his atheism, or his conception of knowledge, but his handling of Marx's dialectical method and its relation to Hegel is nicely done.

The account of Marx's view of morality is perhaps the most controversial part of the book. Wood expounds the position, which he has developed elsewhere, that Marx's condemnation of capitalism is not based on moral considerations at all. In particular, he argues that Marx held that capitalist exploitation is just, because it is a natural consequence of the capitalist mode of production and moral concepts like "right" or "just" receive their only content from the social relations of the time.



Wood breaks little new ground, but he is careful in his analysis, insightful in his commentary, and judicious in his criticism. He pushes no overarching interpretation of Marx; indeed, the five parts of his book could stand independently. He even concedes that the philosophical issues he treats are by and large peripheral or irrelevant to the central tenets of historical materialism or Marx's economic theory. Still, they are necessary topics for comprehending Marx, and Wood's book provides a valuable, philosophically intelligent discussion of them.

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DAVID F. LINDENFELD. *The Transformation of Positivism: Alexius Meinong and European Thought, 1880-1920*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xii, 301. \$25.00.

The thesis of this challenging study is that a revolt *within* positivism, as well as a revolt *against* positivism, formed part of the intellectual revolution that occurred in Europe between 1880 and 1920; that both revolts stemmed from a common preoccupation with the crisis of reason during this period of political and social turmoil; and that both were integrally related in the work of the versatile, but now largely forgotten, Austrian philosopher, Alexius Meinong, who played a role in several intellectual movements, notably analytic philosophy, phenomenology, and Gestalt psychology, which are now thought of as quite distinct and even mutually exclusive. Among those, positivists and anti-positivists alike, whom Meinong anticipated or influenced were Freud, Weber, Dilthey, and Durkheim, who sought new techniques by which to understand the irrational aspects of human behavior and to link them with the rational, and Russell, Frege, Husserl, and Wittgenstein, who sought to redefine reason itself through their investigations of language and its relation to logic and psychology.

The first part of *The Transformation of Positivism* discusses the tradition of British and Austrian empiricism in which Meinong was educated, the political and social conditions of Austria in the late nineteenth century that influenced his thought, and the highlights of his personal life and career. The second part traces Meinong's intellectual development from his revolt against "psychologism," which sought to reduce all intellectual questions to psychological ones, to his new concern after 1900 with language and semantics, culminating in his theory of objects and value. The third part shows how positivism, the new trends in psychology, and phenomenology evolved away from Meinong's original concepts and emerged as distinct, and even opposed, movements by the 1920s.

Lindenfeld makes it clear throughout that, with some reservations, his own sympathies lie with positivism. He rejects the definition of positivism as a philosophy that uncritically applies the prevailing methods of natural science to the study of man and sees it instead as a philosophy based on the assumption that there is a determinate order in the natural universe of which man is a part, and from which he cannot be separated. "To apply scientific methods to man is to affirm that human truth is a part of natural truth, and that the answers to social and ethical questions—such as the proper spheres of freedom and authority—must come from our understanding of the proper relation between that part and the whole" (p. 11). Lindenfeld acknowledges, however, that positivism, like Marxism, can, in the hands of an uncontrolled elite, lead to the sort of totalitarianism envisaged by such anti-utopian writers as Huxley and Orwell. Nevertheless, he recommends as the basis of the only secular humanism viable today the type of positivism represented by Meinong, which integrates the presuppositions of natural science with the humanities, without subordinating one to the other. Whether Lindenfeld proves his case, readers will decide for themselves. But his book is certain to enrich our understanding of the positivist tradition and its applicability to the human condition.

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FRITZ MACHLUP. *Knowledge: Its Creation, Distribution, and Economic Significance*. Volume 1, *Knowledge and Knowledge Production*. Princeton: Princeton University Press. 1980. Pp. xxix, 272. \$17.50.

Why the organ of an association of historians should want to review the volume before us is not on the face of it clear. It is by no means a work of history. Nor is it readily obvious that the techniques and procedures that are used in it can be transferred to the practice of history. To be sure, by most definitions—from the rather broad, even permissive, view of Fritz Machlup to narrower views however motivated—history itself is a form of knowledge. So far as I can tell, only extreme forms of positivism in epistemology—of the sort to which that eminent historian David Hume subscribed—would exclude history from consideration in attempts to characterize ways of knowing. Since Machlup's book is concerned with both the production and dissemination of knowledge, one could suppose that it would have something to say about the work of historians both as providers of new knowledge through scholarly research and publication and as disseminators of established knowledge as teachers of the young.

In 1962, Machlup published a book called *The*

*Production and Distribution of Knowledge in the United States*. I have not seen that book, but it would appear from the way it is mentioned in the present volume that its purpose was to develop methods for the economic study of the production and distribution of knowledge and to offer a cost-benefit analysis of such production and distribution in the United States. Obviously it was necessary to determine what knowledge is, and (perhaps not so obviously) the criteria for that determination had to be tied to the techniques of economics, not those of epistemology. I might say at this point that one of the interesting matters that comes up frequently in the book is how the determination of criteria for inclusion or exclusion are grounded *not* in the veridical apprehension of what things are but in *decisions* determined by the investigator's sense of what his problems are and how they are to be dealt with given the limitations of his procedures. Having produced that earlier book, however, Machlup did not abandon its subject, and what we now have before us is a revised version of that earlier study. It is, however, very much enlarged, and the volume is actually only the first of what is expected to be a work of eight volumes. For the most part, these are yet to be written—although the data on which they are to be based are mainly in hand—and Machlup reserves the right to make revisions in the plan for the whole that he presents in this first volume. Volume 2 will contain material on branches of learning, volume 3 is to be devoted to education, and volume 4 will deal with research and new knowledge, so one may expect that the discipline of history will be dealt with in due course.

I rather suppose that Machlup found this volume fun to write. He takes the opportunity to express his *opinions* on all manner of things that I should suppose economists do not pronounce upon *ex cathedra*—matters dealing with values, public policies, and knowledge. But in the end the book is a work of economics or, rather, the introduction to a work of economics, and whether or not one wants to read it will depend on how interested one is in economic assessments of what one does.

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ALEX BEIN. *Die Judenfrage: Biographie eines Weltproblems*. In two volumes. Stuttgart: Deutsche Verlags-Anstalt. 1980. Pp. xvi, 464; vii, 429. DM 60 the set.

JACOB KATZ. *From Prejudice to Destruction: Anti-Semitism, 1700–1933*. Cambridge: Harvard University Press. 1980. Pp. viii, 392. \$20.00.

For the student of Western civilization the emergence of antisemitism and its persistence into our

own times confounds any easy confidence in human benevolence or historical progress. For the historian the problem is twofold: What are the origins of this peculiar animus against the Jewish people and their religion, and how has this hostility survived despite the change and even the disappearance of the circumstances that spawned and supported it in the past.

Both Jacob Katz and Alex Bein agree that since ancient times the very persistence of the Jews, who were unwilling to renounce their separate identity despite defeat by conquering nations or to accommodate their beliefs to those of surrounding faiths, has evoked the animosity of others. As Bein relates in considerable detail, the Jews' separatism, their insistence on their own identity, was their sin in the eyes of the nations around them. This affront was perceived and treated especially severely by the early church, which portrayed the Jews as denying God through their rejection of his appointed Messiah. Thus the church held that since the Jews had rejected God they were rejected by him, and therefore had to be kept in an abased state as witness to their godforsaken condition. From this they could be redeemed only by recognizing the truth of Christianity. For Jews one saving feature of this theology was its fundamental conversionary intent. The church's insistence that the Jews be maintained as witnesses to their sin made this policy theoretically preservationist rather than destructive; although in practice, as in the anti-Jewish riots during the Crusades or the plague years, the actual consequences could be murderous.

Katz takes this premodern antisemitism as his point of departure and seeks the reasons for its paradoxical persistence, despite the weakening of the Christian matrix that supported it and despite independent efforts since the Enlightenment to improve the social and political condition of the Jews. Katz's answer comes in the form of a chronicle of tenacious anti-Jewish prejudice. The medieval tradition of antisemitism, especially as distilled in the *Entdecktes Judentum* of J. A. Eisenmenger, established an imagery and vocabulary of disdain and condemnation that remained at hand and found employment even by those who might have been expected to be free of its influence. Thus in their attack on traditional religion deists turned against Judaism with special vehemence as the author of the supposedly debased morality of the Old Testament and the forebear of Christianity. Liberals regarded continued Jewish cohesion as a challenge to their own universalist world view and easily resorted to anti-Jewish invective. Socialists found it convenient to condemn the Jews as the very embodiment of the corrupt order they hoped to overthrow. Meanwhile, the conservative and reactionary right throughout Europe resorted to the vocabulary of antisemitism to con-

demn Jewish social and political advances in what they regarded as an exclusively Christian society.

Although these many examples are sometimes only loosely linked, Katz's account gains real focus with the year 1879, which he regards as marking the emergence of antisemitism as a self-conscious, coherent social movement bent on reversing the emancipation of the Jews that had meanwhile been achieved, or at least neutralizing its social effects. At this point "the negative image of the Jew, deeply ingrained in the consciousness of European nations . . . easily reasserted itself" (p. 267). Although exploited equally throughout Western and Central Europe (Katz systematically treats France, Germany, Austria, and Hungary), it found an especially favorable response in Germany, where Hitler made it an essential element in his rise to power.

Katz's primary contribution to our understanding of his subject is his demonstration of the transmutability and persistence of traditional Christian antisemitism even among those who were "unconcerned about the dogmatic truth of Christianity" or, in a secularized form, among those who openly rejected Christianity. "Even if they negated the Christian motives responsible for the creation of the situation [in which Jews were cast as a pariah people], anti-Semites still took it as the basis of their operation." Whether this fact makes "Christianity accountable for all the enormities of modern anti-Semitism, including its culmination in the Holocaust," as Katz states in a concluding excursus on ethical accountability, depends, as he notes, on the historian's evaluation of causation (pp. 319, 321). One would also need a fuller treatment than Katz provides of the period after the First World War, or indeed the period after the 1880s and 1890s, where the bulk of his account ends.

By contrast with Katz's work, Bein's is more general and sweeping. In practice he has defined the "Jewish question" broadly to include not only the relations between Jews and non-Jews through the ages (its common definition) but also Jews' understanding of their own situation (including such matters as Jewish messianism and mysticism) and their relations with one another (particularly regarding Israel and the Diaspora). Thus despite his protestations that he is not writing a general history of the Jews, in places he has done just that. Happily so, since this energetic account will surely engage the attention of the general reader for whom it is intended. Bein's history is very much a personal one, following the contours of his own interest in the subject as much as providing a comprehensive account. (A whole separate volume of asides, excursions, and references preserves the flow of the main narrative.) It is also frankly Zionist, especially in its introductory and concluding sections. Bein regards the anomaly of Jewish existence to have been

that of a people without a land, a deficiency remedied by the establishment of the state of Israel.

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STEIN UGELVIK LARSEN *et al.*, editors. *Who Were the Fascists? Social Roots of European Fascism*. Oslo: Universitetsforlaget; distributed by Columbia University Press, New York. 1980. Pp. 816. \$48.00.

How far, ask the editors of this big book, can quantitative sociological analysis advance us toward a comparative theory of generic fascism? Answer: not far; but the effort here is remarkable. The work grew out of the proceedings of a conference at Bergen, Norway, in 1974 on the social basis of so-called fascist movements in Europe. Added were contributions solicited by an international team of editors from other scholars in Europe and America, and the collective result is fifty articles on fascist movements in twenty-one countries. In contrast to the stress in most texts on the successful parties of Hitler and Mussolini, more room is provided to struggling groups elsewhere, and readers will find studies, for example, on the little-known organizations in Switzerland, Denmark, and Iceland and five articles on the small *Nasjonal Samling* in Norway. Presented as well are the peculiar and commonly neglected movements of Eastern Europe, an area described accurately as "the most deviant sector of the fascist universe." Thus there is abundance here: the fascisms are many, the pages are packed, the print is fine, and statistics fall like rain.

In time this volume is likely to be established as a major reference work on the sociological approach to fascism, and a major achievement of its chief contributing editor, Bernt Hagtvet of the University of Bergen, has been to order a multitude of often disconnected articles around this organizing idea. Much is involved in this choice of approach. In fact the question "Who were the fascists?" is many questions in one: Who were the founders of reputed fascist movements? Who were the members? Who were the sympathizers? Who were the electoral supporters? To attempt to respond to these questions by methods of collective sociological analysis is to seek objective answers, expressed in terms of class, age, occupation, and status. Such empirical studies, explains a contributor, are a recent development, a significant departure from the familiar, emotive literature written in revulsion against the fascist experience. There is no outrage present, no moralizing, and few references to mass murder. Nor is there wrangling over the meaning of the term fascism itself, a vexed issue taken up by some authors in the theoretical section at the beginning of the book but thereafter largely ignored by most others. Rather,

research here reaches a more positivist level—a stage at which scholars, having gained emotional distance from the fascist years, attempt objectively to count, measure, and compare. Their investigations generally confirm that the middle classes and small farmers were overrepresented in most putative fascist movements, that the social composition of membership frequently changed over time, that early joiners were younger than members of other parties (save the communists), and that in many areas voting support dwindled as the economic depression waned, causing some fascist leaders to attempt to imitate more directly the successful organizations in Germany and Italy.

The merit of this book is that most contributors not only recognize the limits of these results but also insist upon these limits. They acknowledge that the question “Who were the fascists?” leads irresistibly to the question “Why were they fascists?” Thus sociology leads to psychology and positivism to political passions. Asks historian John Haag, “How does one weigh or count bitterness, anger, frustration, hopes born of sheer desperation?” (p. 239). Quantitative measurements may provide clues, but other methods must provide understanding. To know the social basis of fascist votes, for example, is not to understand the politics of fascist parties. Nor does evidence of the ascendant support of the lower middle class in some movements prove the popular thesis that fascism was a petty bourgeois revolt. In separate articles Reinhard Kühnl, Alan S. Milward, and Renzo de Felice argue effectively that fascist elites displayed a tendency to act autonomously, unpredictably, without primary regard for the interests of those social and economic groups that provided their support base. What statistics on the overrepresentation of lower-middle-class supporters produce, therefore, are questions that statistics cannot answer. What did members of this class see in fascism? What did they want it to accomplish for them? Why, when it failed to do so in Italy and Germany, did they continue to support the regimes? The consummate task, therefore, is not to count heads but to get inside them.

In sum, this book provides not merely a canvass of a particular sociological approach to fascism but an impressive critique of the possibilities of that approach. In an interesting concluding essay, Peter H. Merkl submits that comparative evidence presented here on the youthfulness of members appears to confirm that fascism was part of a generational revolt; but in the absence of further attitudinal testimony, he remarks, everything else about human recruitment to fascism remains circumstantial evidence. Therefore the question remains: Who were the fascists?

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DAVID E. KAISER. *Economic Diplomacy and the Origins of the Second World War: Germany, Britain, France, and Eastern Europe, 1930–1939*. Princeton: Princeton University Press. 1980. Pp. xvi, 346. Cloth \$25.00, paper \$12.50.

This book is an ambitious general survey of the role of Eastern Europe in the foreign policy of Germany, Britain, and France in the 1930s, with certain chapters paying special attention to the economic aspects of these diplomatic relationships. It sustains the tradition of diplomatic historical method by being thoroughly based on archival research, although in this case the author, David E. Kaiser, had little choice but to confine himself to the archives of the Western countries. Unfortunately it also reveals the insufficiency of diplomatic history alone for dealing with international economic problems. The published work on the terms of trade is entirely ignored, which means that the economic relationships are in fact treated in only a superficial way. Partly because of this the author flounders indecisively between claiming in the earlier part of the book that these relationships were exploitative ones intended by the Nazi government and, in the later part of the book, providing a much more realistic assessment of the relative unimportance of these economic connections to the German economy and demonstrating a certain awareness that in many respects they were the consequences of Germany's international economic weakness rather than its strength.

The book thus represents a gradual struggle toward the triumph of historical evidence over prejudices and assumptions. But the triumph is incomplete because the author then takes refuge in the argument that because the contribution that Southeastern Europe could make to the German economy was so small, Germany was driven inexorably to a war of expansion in order to solve its economic problems. There is no proof at all in this book that Germany's contemporary economic difficulties had any impact on Hitler's foreign aims. The argument fits in, however, with the author's other contention that after 1925 Hitler was inflexibly determined on a war of conquest in the east. The moment for unleashing this, Kaiser argues, was in part provoked by the apparent inadequacy of foreign economic policy. But if these were Hitler's fixed intentions, as they very probably were, that is not substantiated by any analysis of Germany's economic relationships with Eastern Europe. To substantiate the other argument that domestic economic policy could be continued only by a war of conquest it would be necessary to analyze the course and outcome of that policy itself, which the author does not attempt.

If diplomatic history is to take on such topics, its



practitioners will have to master economic history, which implies more than compiling a few tables. The contribution the book makes to the subject is in the comprehensiveness with which it aims to cover the topic. Yet the author might better have confined his obvious abilities to the narrower task that is still necessary. The only way this topic can be advanced is by way of a detailed analysis of each separate bilateral agreement between Germany and its southeastern partners, which would confirm or contradict the present evidence from the terms of trade.

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KENNETH HUDSON. *World Industrial Archaeology*. (New Studies in Archaeology.) New York: Cambridge University Press. 1979. Pp. 247. Cloth \$37.50, paper \$9.95.

"Industrial archaeology," according to Kenneth Hudson, "is the discovery, recording, and study of the physical remains of yesterday's industries and communications." The term was first used in England in the mid-1950s, and Hudson penned the first book to appear on the subject in 1963. Since then, interest in industrial archaeology has spread most successfully to Europe and America, with enough work going on elsewhere to allow the "world" title of this impressive synthesis. Gathering over forty representative studies under eight major categories, the book demonstrates the utility and quality of work being done and, by implication, establishes industrial archaeology as a legitimate professional discipline.

That Kenneth Hudson is the obvious—and perhaps only—author able to undertake such a task is incontestable. Author of numerous books and articles on specific sites as well as the discipline, Hudson is a vocal and insightful spokesman for industrial archaeology. Hudson marshals his enviable worldwide travel and contacts and presents a point-in-time inventory and assessment of the field, which is attempting to establish its academic validity without losing its more popular base. The brief introduction to the book, titled "The Aims and Academic Status of Industrial Archaeology," focuses on a survey of thirteen prominent English archaeologists that establishes little more than an obvious academic uncertainty regarding industrial archaeology. Although this should surprise no one, it seems to frustrate Hudson, who concludes that industrial archaeology is unique, requiring a "special range of disciplines" that marry "scientific investigation with a freely and vigorously functioning imagination" (p. 12).

If there is a low point in the book, it fortunately

passes with the introduction. The first chapter describes the variety of techniques employed by industrial archaeology in selecting, gathering, and presenting its research data with particular emphasis on the use of historic sites as research material. Hudson emphasizes the respectable antiquarian efforts of the Historic American Engineering Record and others in documenting sites, and he adds a light sense of urgency about the need for collecting these records before the sites are destroyed.

With basic techniques defined, Hudson proceeds with models of good work in the field. The next chapter, on "Extractive Industries," examines the synthesizing nature of work in industrial archaeology. M. J. H. Southway's work on the coalfields around Kingswood, England, for example, combined documentary evidence on the area, individual pits, oral history, current examinations of the sites, and a good technical analysis of work methods and processes. The result is good history that explains settlement patterns and local geography as well. Hudson also discusses Le Grand-Hornu in Belgium, an immense workshop and housing complex planned and completed in 1832. Here he also touches on a secondary theme, the significance and utility of saving some of these sites from the bulldozer. Le Grand-Hornu was threatened and then spared, leaving Belgium the richer for it.

Each of the subsequent chapters weaves a similar tapestry, threading historical method and archaeological analysis through the warp of Hudson's broad acquaintance with historic industrial sites. Chapters on "Food and Drink," "Construction," "Metal Processing," and "Transport" carry the reader to twenty more sites, each making a distinct contribution to the reader's knowledge. Although ample photographs and illustrations enrich the text, this is no coffee-table travelogue. Hudson's selections make their point, providing student and professional with lessons in perspective and in the obvious utility of using these structures as research documents. For example, a short section on Buffor's Ironworks in Cote d'Or, France, highlights the pioneering work of Bernard Rignault whose research combines history and archaeology in a masterful interpretation of the site. Although some readers will flinch at Hudson's criticism that "A mere historian of technology would miss what Rignault did not," Hudson admits that "the pedestrian, unimaginative nature" (p. 124) of much work in industrial archaeology leaves him most disappointed.

The last three chapters continue these themes. In "Textiles, Clothing, and Footwear" the reader can explore early Rhode Island textile mills and their architectural antecedents in the village church. A South African woolen mill reveals much about technology transfer and local economic conditions, and two mills in New Hampshire provide a historic



preservation counterpoint. The Clark shoe-manufacturing town of Street in England allows Hudson to stress that research in industrial towns must be open and all-encompassing: selectivity in interpretation can be as unfortunate a bias in industrial archaeology as it is in any other research field. To remind his readers that fitting subjects for industrial archaeology are not necessarily very old, Hudson includes a short section on the archaeology of nylon in England. The chapter on "Power" moves from wind and water to steam and electricity, using five sites in five countries. Hudson's command is again impressive, contrasting straightforward descriptive work with near heroic preservation efforts. Hudson shows the instructive utility of site location maps, the variety of approaches to electrical stations, and the importance of understanding the larger industrial context of any given power plant. In each, he teaches the reader yet another perspective, adds to the vocabulary of approaches, and demonstrates just how industrial archaeology can perform if given the opportunity.

It all adds up to an important book, the only work to attempt such a synthesis on a serious level. For anyone interested in learning more about industrial archaeology, this is fertile ground. For the student, it is a rich and readable introduction. For the scholar searching out unknown manuscripts, Hudson unveils a treasure of overlooked documents: the structures in which our working history left its mark. And for the industrial archaeologist, Hudson creates a respectable synthesis of work in the field. Virtually every major contributor to the field is included—even the most ardent industrial archaeology bibliophile will find new sources in Hudson's text, selected bibliography, and index.

In his brief conclusion, Hudson states what he implies throughout the book. His goal has been more than synthesis: it has been to break the academic conservatism that rejects the "emotion" of industrial archaeology in favor of a "much less dangerous form of scholarship, the history of technology" (p. 235). Hudson wants historians recounting the past also to touch what they can of that past. Industrial archaeology offers a methodology that can serve to make that attempt academically respectable, and Hudson argues for both the attempt and its respectability. For those who deal with technology and call themselves historians, Hudson's occasional slurs are softened by his intelligent assemblage of the reasons why historians should abandon their exclusive dependence on written manuscripts if they claim to have examined all the available evidence.

Although Hudson argues the validity of industrial archaeology as "essentially a field study and a humane study" (p. 236), the work of the authors he has selected makes a stronger, if more subtle, argument. Academic specialization can lead to a safe

and self-satisfying kind of myopia. Industrial archaeology—not as a formal discipline, but as an additional and essential perspective—can assist those who study the working world of the past to avoid such complacent scholarship. In a tone more often helpful and informative than strident, Hudson reminds his readers of a larger, more interconnected world. He demands serious scholarship, counsels the utility of popular treatments when informed by disciplined research, and ties a wide, fledgling field of practice into a unified discipline of considerable promise. No other writer in the field is in a better position to write *World Industrial Archaeology*. Kenneth Hudson matches the responsibility of his position with a well-written and imaginative work.

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OWEN H. WANGENSTEEN and SARAH D. WANGENSTEEN. *The Rise of Surgery: From Empiric Craft to Scientific Discipline*. Minneapolis: University of Minnesota Press. 1978. Pp. xv, 785. \$39.50.

The Wangensteens have produced a comprehensive and encyclopedic series of stories about the evolution of surgical concepts and operative techniques. The late Owen H. Wangenstein has long been recognized as a leading surgical educator and innovator, and the team he formed with his wife, Sarah D. Wangenstein, is widely recognized for its contributions to the history of surgery.

The first fifteen chapters of their book describe the evolution of concepts of wound healing and of several specific surgical techniques such as lithotomy, relief of intestinal obstructions (one of Owen H. Wangenstein's own clinical specialties), excision of ovarian tumors, Caesarian section, and cardiovascular surgery. The next ten chapters deal with more general topics, including the development of anesthesia and the germ theory, hospitals and operating rooms, pre-operative preparation of the patient, surgical specialization, the contributions of wartime surgical necessity, and the complex discoveries and inventions that became possible after the introduction of anesthesia in 1846 and of dependable antiseptic techniques in the 1870s. A terminal chapter summarizes the major events described earlier and looks to the future.

Most readers will find this book useful chiefly as an encyclopedia in which they will find answers to many specific medical problems they encounter in the course of their reading and research. They, however, as well as physicians who are not surgeons, will certainly need a dictionary, an anatomical atlas, or perhaps consultations with a surgeon, to help them understand fully many of the procedures described in the book. Its style is excessively terse, and the book makes many assumptions about how much its

readers already know. For instance, the authors really could have written that Napoleon III had gonorrhea, instead of employing the prolix prudishness of "Neisserian posturethical stricture."

The Wangensteens have included many particularly valuable features. Probably the most important is their reliance on elementary statistics, such as cure rates and mortality rates, in assessing the efficacy and hazards of specific operative innovations. These data will lay many ancient myths to well-deserved rest. The notes and suggestions for further reading, which occupy 137 pages, will provide much information and many clues for both the medical and the general historian, and the detailed indexes will be helpful guides through the sometimes arbitrary sequences in which the book unfolds its stories. Some of the illustrations will engender appropriate feelings of horror in modern readers, while providing relief that so many improvements have occurred since. The relegation of most portraits to the book's ample margins has relieved the tedium inherent in the many medical histories in which the actors and their masks were emphasized at the expense of the play.

Sometimes the authors have been victimized by earlier historians. For instance, Pierre Louis's papers of 1828 and 1835 show that some of his patients did die in spite of bleeding and that he never suggested abandoning that treatment (p. 188). The Wangensteens' chapter on bleeding would have been more useful to historians, and less damaging to medicine's past, if it had discussed the rationale for bleeding as a treatment; as the chapter is, bleeding does appear to have been a ridiculous attempt at therapy, overlooking its reasonable basis in the physiological concepts that prevailed until the nineteenth century. Similarly, evidence for bleeding is scant in the Egyptian papyri (p. 246). And, although there is little written evidence that honey was an effective antiseptic (pp. 316-17), recent data from two laboratories show that it could well have been.

This book will be the most useful available reference source for answering questions pertaining to historic surgery. Although it contains a few errors, many are not attributable to the authors; the correct answers will readily emerge by consulting the sources cited. The book is not easy to read, but no good encyclopedia is.

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J. WAYNE BAKER. *Heinrich Bullinger and the Covenant: The Other Reformed Tradition*. Athens: Ohio University Press. 1980. Pp. xxvi, 300. \$18.95.

Although a well-ordered Bullinger society in Switzerland is currently editing his works, the successor of Zwingli and leader of the Reformed Church in Zu-

rich has until now failed to receive biographic or systematic treatment in English. J. Wayne Baker has done the latter task well by considering the difficult and key issue of Bullinger's use of the idea of covenant.

A bit narrowly conceived along theological-conceptual lines, Baker's six chapters nevertheless offer a clear treatment of Bullinger's federal theology, described as "the other Reformed tradition" in addition to that of Calvin and Reformed Orthodoxy. Recognizing the rivalry between Zurich and Geneva, Baker gives evidence for three basic differences between them. Unlike Luther and Calvin, who held to a unilateral testament view, Bullinger followed Zwingli in introducing a mutual or bilateral covenant idea. Avoiding predestinating testamental implications, Bullinger interpreted single predestination within covenantal terms without falling into a semi-Pelagian stance. Finally, Bullinger developed the Zurich governmental system whereby the Christian magistrate enforced covenantal discipline throughout the commonwealth, unlike Calvin and Beza, who reserved the power of excommunication for the church consistory.

Baker's first chapter considers the beginnings of the bilateral covenant notion in the thought of Zwingli and Bullinger (along with Bucer and the late Melancthon), as well as possible precedents in Christian thought prior to the Reformation, mainly in Tertullian, Lactantius, and Eusebius. Baker's second chapter overstates Calvin's continuity with Reformed Orthodoxy in subordinating the testamental covenant to double predestination (whereas Bullinger's bilateral covenant made God's election binding in history if individuals kept its conditions). The third and fourth chapters thoroughly mine Bullinger's works for evidence on his reading of history from Adam to his own day in covenantal terms. The final two chapters trace Bullinger's application of the covenant to Zurich society and to his refutation of the Anabaptists, who were still more voluntaristic but anarchic. Three excellent appendixes fill in details of larger settings: early Reformation testamental thought, testament and predestination in Calvin, and the defeat of Bullinger's covenant as Calvinistic predestination paralyzed it. But with Cocceius and others in the seventeenth century, Baker argues, Bullinger's conditional covenant came to life again.

An excellent introduction to a difficult and many-sided theme, Baker's work occasionally bogs down in the minutiae of sixteenth-century argument. Its use of Bullinger materials is impressive and seemingly exhaustive, although Baker fails to explain Bullinger's wide influence in his own time, and the somewhat sketchy effort to place Bullinger within the larger doctrinal tradition is unconvincing. One is not sure whether Calvin is presented fairly since, according to Baker, predestination ruled

his thought. At the same time, Baker correctly avoids overemphasizing later covenant theology in terms of a belated Bullinger victory. Yet the argument suggests a Bullinger theology to which the author is committed without presenting clear evidence of its historical accomplishments.

If one follows Baker, the Bullinger bilateral covenant appears to have been largely abortive in spite of its clarity and persuasiveness. Thus Baker's argument fails to convince the reader that Bullinger established a powerful tradition. Larger issues of political, economic, and social concern surrounding Bullinger are not sufficiently considered. One may hope that scholars interested in theology will ponder Baker's pioneer work in English on Bullinger and that it may inspire additional research and still more adequate presentation of a sixteenth-century theologian and church father of more than minor interest and influence.

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LIONEL ROTHKRUG. *Religious Practices and Collective Perceptions: Hidden Homologies in the Renaissance and Reformation*. (Historical Reflections, volume 7, number 1.) Waterloo: Historical Reflections. 1980. Pp. xiii, 264. \$16.00.

The vast chasms that separate areas of scholarly activity have long troubled those who study and try to explain late medieval and early modern European religious culture. Divisions between high and low, learned and unlearned culture, letter and spirit, region and region—let alone faith and works and free will and predestination—have inspired and resisted systematic description even in the best of recent works. How long a time period and what elements within it are required to make sense of the Renaissance? the Reformation? early modern Europe? the whole period connected to the Middle Ages and to later European history? Lionel Rothkrug states his criteria early and bluntly: "This essay argues that the Renaissance, the Reformation and the Counter-Reformation proceeded largely out of regionally distinct sequential mutations in patterns of collective intercourse with departed souls" (p. xi). It is an extraordinary argument, extraordinarily presented.

First, its presentation as an entire number of the journal *Historical Reflections* must be noted, complete with an articulate and defensive preface by John New and appended commentaries by Erik Midelfort and George H. Williams. Such packaging includes some impressive reviews with the book itself, but it also testifies eloquently to Rothkrug's integrity and fair-mindedness. Far less appealing is the fact that the book is badly made and worse edited.

Scholarly novelty and originality are no excuse for sloppy editing and production values; Rothkrug's sledding is going to be tough enough without the editorial problems he now has.

This book is the second stage of a three-stage project. The first stage was Rothkrug's exciting long article in *Religion and the People, 800-1700*, edited by James Obelkevich (1979). The final stage will be a "full-length . . . essay" (p. 187) extending into early nineteenth-century Germany. The present avatar of that sequential mutation revises the earlier article, adds comparative material on France, and deepens and broadens Rothkrug's study of German regional piety on the eve of the Reformation. Part 1 adds the comparative material on France between 500 and 1600, arguing that in France a "moral community" of "transregional loyalties" was formed as people transferred qualities to the king that they had previously attributed to native saints, forming a *religion royale*. Part 2 examines German regionalism in terms of varying forms and patterns of piety, with concentration on prince-bishoprics, the uses of the Inquisition, and the rise of witchcraft accusations. Part 3 offers a comparative structural analysis of witchcraft, controlled and uncontrolled popular devotion, and ecclesiological theories centered on the thesis that "beginning in the twelfth century a fundamental shift in people's perception of their temporal relations to the dead, causing them to pass from cyclical to linear notions of time, contributed to a gradual invalidation of relics and altered profoundly their ideas about death and the Last Judgment" (p. 127). The argument is intricate, unfamiliar, and a little too directly appealing to those who think that social science jargon is a substitute for rational historical discourse. Only the treatment of German pilgrimage sites and cults is based on majestically assembled original research, although Rothkrug has thoroughly read with great originality the best and most recondite secondary scholarship on France.

Everyone (including, now, historians of medieval France) will have some sort of argument with Rothkrug, from nitpicking to metaphysical (and confessional) revulsion. Eliminating both ends of this spectrum, all of these arguments will be productive, both for Rothkrug and his readers. I have argued elsewhere that the social history of heaven is an important missing element in medieval and early modern social history. Rothkrug takes up a similar argument with learning, force, and passion. His book takes an honorable place with the best works of synthesis and with František Graus's *Lebendige Vergangenheit* (1975), the other major and recent important attempt to link early and recent European history through variations of regional loyalties and popular consciousness. This wonderfully disputatious book will, *pace* New, be attacked and cor-

rected for a long time, because it changes the very terms of discourse for the description of early European history.

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ÉMILE POULAT. *Une église ébranlée: Changement, conflit et continuité de Pie XII à Jean-Paul II.* (Religion et Sociétés.) Paris: Casterman. 1980. Pp. 303.

The articles in this anthology, as the subtitle of the volume indicates, focus on change, conflict, and continuity in the Roman Catholic Church during the last forty years and examine developments from the pontificate of Pius XII to that of John Paul II. The author, co-editor of the Casterman series "Religion and Societies," in which the present volume is published, has written more than a dozen books on the church in the modern world. Among his monographs are works on modernism and the worker-priest movement. The latter provides the theme for more than a third of the fourteen articles in this collection, most of which concentrate on the church in France.

All of the articles included here have been published elsewhere during the course of a quarter of a century. They vary in subject, length, and value. Although they have been reviewed and retouched, some more revised than others, they have not been substantially altered. They remain what they were originally—a series of pieces on the church in France that were meant to stand on their own without much coordination. To be sure, a certain unity stems from the fact that they are all the work of one individual, whose view of the church has been formulated over the years as he has watched an agitated church attempt to accommodate itself with a troubled world.

Rather apologetically, Émile Poulat acknowledges that he did not intend to write articles—he prefers longer, more detailed studies—and did not foresee that they would be published in book form. Indeed his introduction, which attempts to put what follows in perspective, is entitled "An Involuntary Work." Although the articles vary in quality, they all suffer to some degree from the fact that the Vatican archives remain closed for these years and only recently have been opened for the pontificate of Leo XIII. In the first chapter the author seeks to provide a survey of the forces from 1939 to 1968 that stimulated change within the church. Within these pages he stresses the impact of the world war, the peace, the division of Europe into two blocs, the emergence of the Third World, and finally the Second Vatican Council. The book's title leads the reader to expect an examination of developments within the universal church, but in

fact Poulat turns to what he knows best and interests him most, Catholicism in France.

The organization of the chapters in the volume confirms Poulat's preoccupation with Catholicism in France. Thus he has written on French Catholics in Germany during World War II, Catholicism in France from the liberation to the Cold War, the religious impact of the war in Algeria, the French bishops on the eve of the Ecumenical Council, and Monsignor Lefebvre's crusade to preserve traditionalism. The greatest coverage is accorded the worker-priest movement that inspired five of the articles. It is true that in the concluding chapter, "The Year of Three Popes (1978)," the author does attempt to broaden his perspective and examine events within the universal church. Nonetheless these pages and others scattered elsewhere that provide a broader view are too few and far between to justify the title.

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#### ANCIENT

ELLEN MEIKSINS WOOD and NEAL WOOD. *Class Ideology and Ancient Political Theory: Socrates, Plato, and Aristotle in Social Context.* New York: Oxford University Press. 1978. Pp. x, 275. \$17.95.

Ellen Meiksins Wood and Neal Wood's book contends that it is essential to understand the social and historical context of the Socratics, the interaction between aristocratic milieu and democratic, Athenian polity, to appreciate correctly the implications of their political theory. In this way the authors emphasize and try to explain a contempt for democracy as the ideological core of Socratic thought. Hardly abstracted from politics, the Socratics were partisans in the contentions of their time; and the aristocracy, morally regenerated by Socratic teaching, was their favored side.

The antidemocratic bias of at least some of the Socratics will come as news to few; and the Woods' citations of recent scholarship show that the need is not as urgent as they insist to warn against uncritical acceptance of Socratic views as either typically Greek or the standard by which to judge the actions and values of Greeks. What gives their book its interest is the authors' attempt to connect Socratic views into a coherent system consistently at odds with the "democratic perfection" of the *polis*.

The approach and the thesis are worth arguing, and the presentation is often engrossing and richly textured. Yet it is constantly bedeviled by a loose use of language that ultimately ruins any confidence in the Woods' point of view. Even so funda-



mental a word as "aristocratic" is never defined or used precisely. It gets associated with words like wealthy, conservative, hierarchic, and oligarchic as convenience prompts, in spite of the fact that they can be shown to apply to democrats as well and do not fit all "aristocrats." How useful as a class denominator is this for a group including Alcibiades and Pericles, Plato and Demosthenes? More basic still, why claim democracy was the essence of the *polis* in general when the very delineation of Athenian political development makes clear Athens's individuality?

This use of language and argument, in addition to overstatement of historical evidence especially on the size and importance of commercial interests, leads to facile characterizations and permits the alleged partisanship of the Socratics to emerge. It is one thing to argue that the Socratics were partisans and another to argue that they were partisans of the aristocracy as contemporaries would have understood that term. How many aristocrats listened to them? At least one Socratic, the one who cared to be a historian, praised the Athenian democracy unequivocally as the most stable polity of his time. Socrates treated oligarchs and democrats equally.

The critique of Greek society found in Socratic texts is perhaps better understood as an interaction with values shared by both aristocrats and democrats. The dialectic between Socrates and his students and those around them is more complex than this book and its own partisanship will allow.

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PETER WALCOT. *Envy and the Greeks: A Study of Human Behaviour*. Warminster, England: Aris and Phillips. 1978. Pp. vi, 120. \$16.50.

Peter Walcot, a classical scholar, explores the significance of envy in ancient Greek society in this short monograph. Having written an earlier book on the Near Eastern background of Hesiod's poetry (1966), he now turns to a Hesiodic subject (compare *Works and Days*, 11. 11-26) that runs through Greek civilization to the end of antiquity. Using a research technique familiar to classicists, the word study, he assembles a large and diverse body of material from Greek texts on *phthonos* (envy) and *zelos* (jealousy) and arranges his discussion partly by chronology, partly by theme, as the material permits. He illuminates the subject of envy in Greek views of the Eastern potentates, the Greek tyrants, the politics of the fifth and fourth centuries, the evil eye, and Christian and later Greek writers, ranging from Homer and Hesiod through Nonnus of Panopolis of the fifth century A.D. The discussion occasionally introduces the insights of social anthropology.

Walcot is able to demonstrate successfully the prevalence of envy in Greek society. The emotion has both good and bad sides. He shows how the Greeks felt that it was inherent in human character. Sexual envy and sibling rivalry abound as folk themes in Greek mythology. In promoting a healthy rivalry among professional people, envy nourishes competition and is at the heart of free enterprise. Conversely, the notion of divine jealousy pervades Greek thinking on theology, and the human counterpart of this is an arrogant pride, suspicion, and malevolence. In political terms envy is perhaps institutionalized in ostracism, a well-known and sometimes puzzling feature of Athenian democracy in the fifth century. Furthermore, Walcot points out (p. 57), the monumental building encouraged by democratic politicians represented what anthropologists call "sop sharing," aimed at relieving the envy felt by the ordinary Athenian for the high and mighty.

In a society obsessed with personal prestige and the pursuit of honor, envy is the dark side of the coin. Christian authors, recognizing man's envious nature quite as well as pagan writers, sought to allay its harmful potential by preaching humility as a cardinal virtue.

Walcot provides a useful index of passages from Greek and Latin authors. The book succeeds in demonstrating the wide range of influence of envy in thought and action in ancient society but is less successful in showing how envy functioned in society (a secondary objective, page 7). I believe this results from the associative, somewhat rambling character of the discussion. The book is provocative, but it is hard to envision its intended audience. There are no footnotes, and authors are introduced as they come up for discussion, as if many ancient writers might be unfamiliar to the reader. A bibliographical chapter is appended for further reading. Classicists will sometimes be taken lightly over familiar territory but will be stimulated by the anthropological reflections. The book provides a useful overview of material that might be investigated with profit along more specialized lines.

WILLIAM C. WEST III

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J. T. HOOKER. *The Ancient Spartans*. London: J. M. Dent and Sons; distributed by Biblio Distribution Center, Totowa, N.J. 1980. Pp. 254. \$29.50.

The history of ancient Sparta has been the subject of much discussion among ancient historians in the last decade or so. There have been several books devoted to all or parts of the Spartan experience and some important works in which discussion of Spar-



tan life and institutions plays a significant part. They have enriched our understanding of that unique, important, and influential state, raising many questions and pointing out many problems. This would appear, therefore, to be a good time to undertake a narrative history that would digest and assimilate this valuable new work.

J. T. Hooker, however, though his useful bibliographies list most of the important recent studies, assigns himself a more modest task: "I have tried in this book to give an introduction to the most important aspects of Spartan life and history down to 371 B.C." The word "introduction" must be taken very seriously, since, for the most part, the depth of narrative and explanation is little, if at all, greater than that to be found in a standard textbook of Greek history. Exceptions to this general statement may be found in the chapters on the topography and archaeology of Sparta and Laconia, which are full of material not well known to most scholars, and in the discussion of archaic literature and music. Discussion of the origins and character of Spartan institutions are less satisfactory, but the least useful part of the book consists of the chapters narrating the history of Sparta in the fifth and fourth centuries. These chapters tend to be little more than brief summaries of Herodotus, Thucydides, and Xenophon, with occasional comments.

Throughout, little is said about factions within Sparta, the relations between Sparta and its allies, or the relationship between internal politics and foreign policy. The author's interest and energy seem to flag as he goes on, for the section on the Persian Wars is better than that on the Peloponnesian War, while the fourth century, the period of Sparta's imperial hegemony and ultimate decline, earns no more than seventeen pages of cursory narrative. The much-needed narrative and interpretive history of ancient Sparta still awaits an author.

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JOHN BUCKLER. *The Theban Hegemony, 371-362 B.C.* (Harvard Historical Studies, number 98.) Cambridge: Harvard University Press. 1980. Pp. x, 339. \$25.00.

To fill a gap in scholarship on fourth-century Greece, John Buckler has produced a painstaking study of the short period during which Thebes gained ascendancy in central Greece. Before 371 Thebes revitalized a Boeotian confederacy and supported the Second Athenian Confederacy in ending Spartan domination. Epaminondas, a gifted Theban general, led Boeotian troops in defeating the Spartans at Leuktra (371) and then in three invasions of the Peloponnese drew together an anti-Spartan alliance among Greeks formerly dominated

by Sparta. A younger Theban commander, Pelopidas, extended Theban influence northward, forging alliances with the Thessalian confederacy and a Macedonian faction. Theban hegemony ended with the battlefield deaths of these brilliant leaders: Pelopidas fell in 364 while leading his hoplites uphill to victory, and Epaminondas was mortally wounded in 362 just as his deep Theban phalanx shattered the Spartan line.

Buckler has traced these two careers, interweaving precise details of the campaigns and diplomatic negotiations, illuminating the story with original photographs and fine maps. His narratives of the military engagements at Leuktra, Kynoskephalai, and Mantinea, based on careful personal observation, are significant in understanding the subsequent development of Macedonian and Hellenistic military strategy. It was the military ingenuity of Epaminondas that ended the myth of Spartan invincibility and prepared for the conquests of Philip and Alexander.

Even more useful to specialists are Buckler's efforts to clarify relationships between cities. Buckler sees Thebes as a champion of democracy, although it accepted some oligarchic states as allies. Eventually the Thebans dismantled the walls of some Boeotian oligarchies, but in this and other actions Thebes was as inconsistently democratic as Athens had been. Although he postulates more democratic features in Boeotian federal machinery than other scholars have thought likely, Buckler is convincing even when abstruse. He notes that Thebans did promote federalism that led to later Hellenistic leagues.

Supplementing the narrative are a full bibliography and valuable appendixes seeking chronological precision and analyzing ancient sources. Buckler shows Xenophon's pro-Spartan bias to be balanced by the more admiring treatment of Epaminondas and Pelopidas in the later writers Diodorus and Plutarch.

In the short epilogue the author ventures some debatable judgments. Is it fair to fault Thebans for failing to formalize their hegemony over a mercurial alliance with small states still under Sparta's shadow? Is it realistic to envision a fourth century "synod of allies" (p. 223) capable of becoming a "tribunal or arbitral court for the resolution of internal disputes"? If Thebans "preferred to deal individually with their allies," were they not following the policy that Rome later employed in forging an empire?

The solid scholarship of this volume should make it a basic resource for study of diplomatic and military development in the ancient world.

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PIERRE BRULÉ. *La Piraterie Crétoise Hellenistique*. (Centre de Recherches d'Histoire Ancienne, number 27.) Paris: Annales Littéraires de l'Université de Besançon. 1978. Pp. vi, 190.

In this book, an abridgment of a doctoral thesis, Pierre Brulé seeks to establish to what extent the Cretans deserved their reputation for piracy and to place their piracy in its social and political context. It is no easy task since our only sources of information are haphazard inscriptions, always formulaic and often mutilated, and casual remarks dropped by Greek historians, the best of whom, Polybius, had no love for Crete.

Brulé divides his treatment into three sections of equal size. In the first he combs the evidence for Hellenistic piracy in general, particularly acts connected with the so-called First and Second Cretan Wars (205, 201 B.C.), to see whether Cretans were actually involved as much as charged: he concludes that they probably were. In the second, dealing with the involvement of Crete's city-states in piracy, he takes up in great detail all inscriptions recording interstate agreements in which asylum, reprisals, reciprocal citizenship, and the like are mentioned. His conclusion is that such fine-sounding agreements were the only recourse people had against piracy. They did not help much, but they do reveal an improvement in attitude: by Hellenistic times some Cretan states were at least willing to enter into agreements designed to restrain piracy. In the third section, Brulé tries to place Cretan piracy in the total picture of Greek piracy. Many Greek areas sent out pirates; why Crete more than the others? For the same reason, he answers, that they furnished so many mercenary soldiers: the land was poor, it was held mostly by an aristocratic upper crust, and the birth rate apparently did not drop as elsewhere—so Crete's poor perforce sought a living abroad.

In the course of his exposition, Brulé straightens out a number of details (for example, he seems right in claiming that the total of four thousand Roman citizens reported as being in slavery in 189 B.C. refers to all of Crete and not merely Gortyn [p. 26]; in emphasizing that the term "Crete" is an abstraction, that the island housed a collection of independent city-states with very different political allegiances [p. 50]; in dating a treaty between Rhodes and Hierapytna to 205 rather than 201 [p. 54]). The exposition itself, based on an exhaustive study of all the literature, both primary and secondary, does not materially change the picture historians have painted but does confirm and enrich it.

Brulé's exposition is not easy reading, however. The book is in the form of a typescript photocopy and is drawn up like a presentation in a college outline series: there are main sections, subsections, and sub-subsections marked off with Roman and Arabic

numerals, capital and small letters. Greek words are transliterated into Roman letters in a weird fashion all the author's own and liberally larded with mistakes. There is an index, but do not trust it.

LIONEL CASSON

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A. J. HEISSERER. *Alexander the Great and the Greeks: The Epigraphic Evidence*. Norman: University of Oklahoma Press. 1980. Pp. xxvii, 252. \$29.95.

"My purpose in this book is to accomplish three goals concerning the most important inscriptions contemporary with the time of Alexander the Great: to give the most accurate text possible for each fragment or stele, to determine as precisely as possible the right date for each inscription, and to arrive at the proper historical context from an analysis of the content of the inscriptions combined with the ancient literary sources."

Thus A. J. Heisserer states succinctly his aims in this volume, and it is a pleasure to report that in most respects he succeeds. This is a study—based on epigraphical evidence—of Alexander's relations with Athens, Eresos, Chios, Mytilene, Priene, Iasos, and Tegea. Some of the inscriptions are well known, others not so, even to the Alexander specialist. In each case the text is painstakingly restored and translated, and we are given photographs of the main stones. Unfortunately, the quality of the plates is uneven, and many are virtually useless; whether this is the result of poor original photography or inferior printing the reviewer is unable to judge.

The general conclusions that emerge from an analysis of these inscriptions is that the Greek states of the eastern Aegean were attached to the so-called League of Corinth; this was the means by which Philip II and Alexander extended Macedonian hegemony into that region. The formal arrangement was maintained as long as necessary, but Alexander increasingly (during the Asia Minor campaigns) found it more convenient to employ extralegal means to deal with the Greeks, treaty provisions having become an encumbrance to the king. By 324 (the Tegean exile-decree) only the will of the king remained.

Among the interesting items is the suggestion (based on *IG II<sup>2</sup> 329*) that Alexander, like Philip before him, had made a separate *symmachia* with Athens. We are also given an account of Gorgos, an influential citizen of Iasos resurrected from obscurity through inscriptions, who served as Alexander's agent in his home town, thereby casting new light on one informal technique by which Alexander controlled the Greeks.

Whatever Alexander's personal predilections

about Hellenism as a cultural force, he treated Greeks on both sides of the Aegean as a conquered people. Heisserer's work, carefully constructed and tightly argued, makes a valuable contribution toward our understanding of that aspect of Macedonian imperialism. It also establishes some thoughtful distinctions between the inherent qualities of literary and epigraphical evidence.

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C. J. EMLYN-JONES. *The Ionians and Hellenism: A Study of the Cultural Achievement of the Early Greek Inhabitants of Asia Minor*. (States and Cities of Ancient Greece.) Boston: Routledge and Kegan Paul. 1980. Pp. x, 237. \$30.00.

Other volumes in this series announce their subject in terms of cities and areas (for example, *The Dorian Aegean, Argos and the Argolid*). Ionia can be understood as a distinct area in Asia Minor, but it is open to doubt whether one should speak of Ionians in the classical age as distinct in *genos* or *paideia* from Dorians and Aeolians. The author's eleven-page bibliography does not mention the important book of Edouard Will, *Doriens et Ioniens* (1956).

C. J. Emlyn-Jones maintains that the Ionians were "undoubtedly different" from their fellow Greeks (p. 8) but never says directly what he means by calling them Ionians. He is prepared to accept the traditional account of the Iron Age migration from mainland Greece, although he avoids any clear statement about the way in which Greek writers indulged their imagination and their local patriotism in describing how and when different cities were founded. His two introductory chapters will only confuse readers by offering them sentences like: "Our knowledge of the Ionian migrations is full, from a personal and genealogical standpoint which possibly reflects the known predilection of early historians for local *ktiseis* (foundation stories)" (p. 11).

He speaks, inaccurately, of the alphabet possibly being invented in Ionia (p. 6) instead of explaining that the Greeks improved on the invention by using certain letters for vowels. He fails to mention that it was the Ionian form of the alphabet that was introduced in Athens in 403 B.C. and that this is the alphabet still used today. He has numerous references to the Ionic dialect but does not take the trouble to write the few sentences that could explain, even to a Greekless reader, how this dialect differed from Doric. Nor does he notice that Ionians called themselves *Iawones* when they first came to Asia, so that the Persians called them, as they called all Greeks, *Yawanas*.

In chapter 3, which considers the character of Io-

nian art and is illustrated with eighteen plates, the author writes with greater authority, and his account of painting and sculpture in Greek Asia Minor should encourage readers to pursue the subject further. But the rest of the book is disappointing. Chapter 4, "Homer and the Ionian Poetic Tradition," offers a conventional appreciation of Homer and attempts to set forth the present state of the Homeric question. There follow two chapters on Ionian philosophers that (like most attempts to present a summary account of the pre-Socratics) will succeed better in puzzling readers than enlightening them. And the brief final chapter, concerned with the Ionian legacy to Hellenism, offers only some miscellaneous pieces of information.

The author writes in an English style that has little grace, and his translations from the Greek are mostly of poor quality. In well-known lines Mimnermus says that he dreads the approach of old age, "which makes a man ugly and useless." The translator who renders these adjectives as "shameful" and "base" (p. 92) not only misunderstands the Greek but also shows little respect for the feelings of a septuagenarian reviewer.

LIONEL PEARSON  
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GETZEL M. COHEN. *The Seleucid Colonies: Studies in Founding, Administration, and Organization*. (Historia, Einzelschriften, number 30.) Wiesbaden: Franz Steiner. 1978. Pp. xiv, 95. DM 28.

This work reflects the increasingly pronounced predilection among scholars of the Hellenistic world for extremely detailed studies on rather narrow topics: here we are presented with an analysis of the ancient source material and modern views about the ambitious Seleucid policy of settling Greeks and Macedonians within their far-flung empire.

Getzel M. Cohen concentrates his attention on the early phase of Seleucid history, that is, from about 312 to 188 B.C., and is especially concerned with problems associated with the identification of the colonists, the manner in which they received the essential land lots (*kleroi*), and the civic structure that they brought to their settlements or developed there. The evidence is sparse, but Cohen convincingly demonstrates the process by which these foundations, essentially Greco-Macedonian islands in an Asian sea, were organized. Invariably founded as garrisons or military colonies, they occasionally developed into the great urban centers of the Roman Empire—Antioch-ad-Orontem is a case in point. The Seleucids attempted to maintain a homogeneous populace in these settlements, which, however, could lose their exclusive character if a new *polis* was created through amalgamation of the

military colonists with the native inhabitants of the surrounding areas. The major inducement, of course, for the settlers was land located in or near the new site. On the basis of comparative analysis, Cohen suggests that the allotments were distributed on the basis of not only rank and military division but also "the relative arrival time at the colony" (p. 58). On the other hand, although at first the land was apparently inalienable, since usually it was created from royal estates and carried specific obligations, eventually it was regarded as private property. Quite striking is the wide prevalence of civic associations (*koina*), which both provided for the social and religious needs of the settlers and served as the basis for the civic arrangements that we see in greater detail whenever these colonies evolved into *poleis*. In all this Cohen has performed an invaluable service for scholars in Hellenistic history, even where they may choose to disagree with him. One especially welcomes two appendixes, each at the end of separate chapters, that discuss the content of certain recently discovered inscriptions from Denizli and Failaka.

In light of the author's thorough presentation of individual aspects of Seleucid colonization, it would be petty to register disagreement over minor points. I have, indeed, only one complaint, which concerns a curious stylistic feature. Despite the fact that Cohen strives to employ consistent British orthography ("honour," "favourite," "connexion," "centre," "signalled"), some oddities protrude, for example, "transferal" (p. 30), "to insure" (p. 35—but "to ensure" at p. 86), "to fulfill" (p. 69), "defense" (p. 88), while "Idumaeans" appears alongside "Judea" (pp. 75 and 80). These items, however, are mere quibbles, and in no way seriously slight this commendable volume. Scholarly and reasonable, it will be a useful tool for future laborers in the vineyard of Hellenistic culture.

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J. F. LAZENBY. *Hannibal's War: A Military History of the Second Punic War*. Warminster, England: Aris and Phillips. 1978. Pp. xiii, 340. £12.50.

The decisive round in the struggle between Rome and Carthage began in April or May 219 B.C. when Hannibal attacked and besieged Saguntum, a town in Spain under Roman protection. Almost certainly, Hannibal's intention was to start a general war: he was ready to invade Italy by June 218 with an army so large and plans so elaborate that they could scarcely have been improvised in the few weeks that intervened between the Roman ultimatum and the start of the campaign. By then, two consciously imperial powers had collided in war over Sicily and in Spain, while their diplomatic ri-

valries encompassed much of the Mediterranean world. Hannibal's campaign was clearly an attempt to use present force to avert an unfavorable future by disrupting the process of strategic assimilation that was making Rome so strong—the process whereby the peoples of Italy had already been subjected as military satellites to provide forces for more expansion.

Hannibal's grand strategy was to enlist the support of the Celts of the Po Valley and such other peoples of Italy as were willing to fight to secure their independence from Rome. Hence the necessity of a southward march toward Rome across northern and central Italy (and of the further movements back and forth across south-central Italy). This in turn called for an overland advance from the Ebro River line in Spain and across southern France with the spectacular crossing of the Alps. It is still widely believed that it was Roman sea power that imposed the overland march, but that ignores the nature of sea power in the ancient world: galleys, whose rowers needed frequent rest and which required resupply almost every day, could not establish a Mahanian naval supremacy over the open sea. We know Carthaginian reinforcements reached the coasts of Italy by sea unhindered in 217 and again in 215, and Hannibal could no doubt have gone by sea if his grand strategy had not imposed the overland route.

Hannibal's talents at the level of theater strategy are well proven by his swift movement from Spain all the way to Italy; his mastery of the operational art of war is revealed by the regularity with which he outmaneuvered Roman armies that tried to intercept him; and his tactical brilliance was demonstrated in virtually every battle with Roman forces—Cannae remains until this day the very model of a battle of annihilation. Thus Hannibal secured a deserved reputation as the greatest military leader of the ancient world but for Alexander—and ultimately like Alexander he was a failure by the severe standards of consolidated achievement that the Romans later set.

Books about Hannibal and on detailed aspects of his warfare by both historians and enthusiasts continue to appear with regularity. Since there are no archaeological or epigraphic sources, all these must be based on Polybius and Livy, together with a collection of later writings of much less persuasive authority. J. F. Lazenby's *Hannibal's War* aims at a step-by-step description of the campaign based on a detailed exegesis of the sources; Lazenby offers no new major interpretations as he himself notes (p. viii), nor does he offer any sort of strategic analysis. But that is not his purpose. His goal is to tell us what happened as straightforwardly as possible (p. ix). Lazenby must therefore be judged on his ability to extract a persuasive stream of evidence from often fragmentary and frequently contradictory



sources. This reader believes he succeeds, but only because Lazenby presents his full exegesis in the main body of the text, allowing us to make our own choices when the evidence is divergent; we do not therefore depend on his judgments.

Certainly Lazenby gives us reasons to be diffident of his opinions. For example, he cites Tzetzes (the Byzantine grammarian of the twelfth century A.D.) on a point of Spanish geography (p. 23), when it is evident that one would not trust that writer to know where Spain itself might be found. Lazenby himself notes as much, thus making the citation pointless. And again on the subject of the elephants, Lazenby asserts that they were likely African and small (the "forest" elephant) rather than Indian (p. 15), a thing basically implausible since the east-west travel from India to Syria was far more easily accomplished than a crossing of the Sahara. Of more consequence is Lazenby's weakness in defining the tactical role of the various types of Roman and Carthaginian forces involved in the fighting: he misunderstands, for example, the purpose and meaning of light infantry, which was primarily a *missile infantry* armed with light spears, slings, and bows.

Against this, *Hannibal's War* is well justified because it is a fully explicated narrative presented in all the detail that our sources (and unchanging geographic references) allow. In English, at any rate, there is now no better account of a slice of history that continues to fascinate.

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J. RUFUS FEARS. *Principes a Diis Electus: The Divine Election of the Emperor as a Political Concept at Rome.* (Papers and Monographs of the American Academy in Rome, number 26.) Rome: American Academy in Rome. 1977. Pp. xxiii, 351.

This is a provocative book on an important subject. Unlike many of its companion volumes produced at the American Academy in Rome, it is also eminently readable, a testimony to the author's considerable literary skill. The title is somewhat misleading. J. Rufus Fears deals in reasonable detail with the ancient Near Eastern and Hellenic background to the theory of the divine election of earthly rulers and includes a discussion of the concept during the Roman Republic. Not until page 121 does he begin to analyze the imperial period of Roman history.

Fears rightly observes, "A belief in the divine election of kings has been of the greatest importance for political theory in both the western world and in other cultures" (p. 2). His thesis is clearly stated: "The distant origin of the idea of *rex Dei gratia* may lie in the ancient Near East. However, the essential basis of this theory of monarchy, which has had

such a profound influence upon the political philosophy of Europe, was the fusion of the Christian conception of kingship with the idea of the election of the emperor by the gods of the Roman state" (p. 7).

In ancient Egypt pharaoh was a living god, not a mortal elected by the gods as their representative, but in Mesopotamia, although theocratic kingship emerged, the king "was not a god but a mere mortal elected by the gods of the state to function as their viceroy on earth" (p. 21). Even the kings of Persia claimed to be "the divinely elected vice-regents of Ahura Mazda" (p. 25). Fears's view that these ancient Near Eastern precedents helped to shape the later Roman theory of divine election, after Roman power absorbed the eastern Mediterranean, is undoubtedly right.

Homer refers to the bestowal of kingship on mortal rulers in both the *Iliad* and the *Odyssey*, and, as Fears shows, "It is important to emphasize that here, at the beginning of the history of divine election in the Greco-Roman world, the king's election by god was directly linked to his obligation to rule wisely, justly, and for the benefit of his subjects" (pp. 30-31). But Greece did not develop theocratic monarchy, and "Homer's clearly articulated view of the divine election of kings became an increasingly faint echo" (p. 34). Only with the emergence of Alexander the Great does the concept of divine election reappear in the context of "divinely granted victories in the field" (p. 45). The great generals of the late Roman Republic presented themselves as divinely favored leaders (p. 111).

For the Roman emperors there is obviously an abundance of evidence reflecting theories of their divine election. Fears demonstrates admirably his familiarity with that evidence and discusses it with boldness—sometimes too much. I am not convinced that Pliny was "a consistent proponent of the theocratic nature of Trajan's principate" (p. 151) or that Trajan's principate was theocratic. Fears sometimes attaches too much significance to his subject despite his occasional disclaimers (for example, that "in the early days of the principate, Augustus' predominance was clearly based on armed might" [p. 216]). Still, it is a good book, well worth reading.

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BRUCE W. FRIER. *Landlords and Tenants in Imperial Rome.* Princeton: Princeton University Press. 1980. Pp. xxxii, 251. \$17.50.

Bruce W. Frier's *Landlords and Tenants in Imperial Rome*, dedicated in German to Max Kaser and the Institut für Römisches Recht at the University of Salzburg, is doubly interesting. First, an American monograph on Roman private law must be hailed with delight because, despite a leading position in



classical studies for the United States, American scholars have not shown much interest in Roman law up to the present time. Second, the monograph is not an exhaustive study on Roman *locatio conductio rei*, for which numerous studies exist from the past century onward, especially in German, but stresses the historical basis of Roman tenancy and its economic and social aspects. The book is remarkable and Frier's method praiseworthy because Roman law, as it is interpreted from two or more millennia, no longer offers itself easily to an original investigation. The *Realien*, as the Germans say, should now be the actual interest in studies of Roman legal texts. This provocative book is a successful specimen of *Realien*-research.

Frier, who wished to write on Roman law but did not desire to deal with an exclusively legal subject, chose to examine the law of tenancy, a topic that combines legal, economic, and social interests. While residing in Rome, he became aware of the rented houses discovered during the excavations at Ostia (*habent sua fata libelli*). The tenants of these apartments were members of the upper—that is, rich—classes. This was an unexpected discovery contrasting with Athens where everybody lived in a privately owned home. (I am well aware that classical Athens cannot be compared with imperial Rome.) Since there was a market for rented accommodations, the decisions of the courts and opinions of Roman jurists played a subsidiary social role in controlling and regulating tenancies. How they went about this forms the major topic of this book. It is not the first time that the excavations at Ostia compel us either to revise or to see from a new standpoint the teaching of classical Roman law. (Another instance happens to be the *possessio*, which inscriptions at Ostia present in a different light from the Roman jurists.)

Frier purposely does not deal with *locatio conductio rei* of the lower classes, mainly that of the proletariat in Rome—a pathetic and tragic image—nor with the laws of leasing found in the papyri of Roman Egypt. Precisely because the book is original and opens new avenues for the study of Roman law, I hope the author overcomes his expressed scruples and extends his study to the lower classes and Roman Egypt.

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PETER MARSDEN. *Roman London*. New York: Thames and Hudson. 1980. Pp. 224. \$19.95.

London was the largest and most important city in Roman Britain, yet little has been done in the past to examine the remains that are deeply buried un-

der the medieval and modern metropolis. In 1973, however, a new Urban Archaeological Department was established as part of the Museum of London, and since then several large and detailed excavations have been carried out, not all of them yet published. This book includes some of the early results of this work, although the author's modest objective is to attempt a reconstruction of this early period in the history of the city. It is well written and is extremely well presented with excellent photographs and ample plans and drawings, including some helpful reconstructions. The account is fully documented with footnotes and a bibliography.

Peter Marsden raises some pertinent questions about the state of the Roman city, not all of which are satisfactorily answered. He does not, for example, accept a military origin at the period of the conquest in 43 A.D. but argues that the city was founded by merchants as part of a plan for the rapid romanization of the new province. There would certainly have been a rapid commercial growth at this key site once wharfrage had been constructed along the river banks and communications opened up to the South and the Midlands. But there were years of conquest ahead and only a small area in the Southeast was effectively pacified. The military importance of the site is not fully appreciated by the author, nor is the evidence given its full weight. The other period over which Marsden expresses considerable doubt is that of the late second and third centuries. He argues that the city never recovered from the great fire of ca. 130 and that there is a conspicuous lack of buildings of this later period. Yet when defenses were erected ca. 200, a vast area of 330 acres was enclosed, and on the analogy with other British settlements it is not likely that it was filled with large empty spaces. It is also certain that there is enough wealth to provide large sculptured monuments, fragments of which have survived.

The evidence from recent excavations and those still in hand will need to be carefully studied to find explanations for this evident paradox. The book is interesting and useful in presenting a picture still in the process of formation and at least serves as an important interim statement and whets an appetite for more to come.

GRAHAM WEBSTER  
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MICHAEL STAHL. *Imperiale Herrschaft und provinzielle Stadt: Strukturprobleme der römischen Reichsorganisation im 1.-3. Jh. der Kaiserzeit*. (Hypomnemata, number 52.) Göttingen: Vandenhoeck und Ruprecht. 1978. Pp. 191. DM 36.

This is a reworking of Michael Stahl's dissertation of 1975. His theme is the relationship between the

central government and provincial cities of the Roman Empire in its first three centuries. Its importance emerges when we understand the conscious efforts of the emperors from Augustus through Severus toward urbanization, particularly in the Rhine and Danube basins, Numidia (which went from twelve to thirty-seven *municipia*), and the Orontes valley in Syria.

Stahl asserts that twin processes were in motion concurrently. In the first, the inherited provincial aristocracies gradually were absorbed into the senatorial and equestrian classes in the capital and attained magistracies, thus widening the basis of power to be more representative of the whole empire-commonwealth. The second involved the gradual decline of the cities from relative autonomy to territorial administrative units of the central government.

The difficulty of pronouncing dogmatically on the political function of the cities and their relationship to Rome arises because they had no single legally defined status. It was inherent in the Roman system from the early republic to establish treaties with client kingdoms, tribes, or towns on an *ad hoc* basis. Towns were thus *liberae et immunes* (free of the provincial governor and exempt from taxes), *stipendiariae* (tribute-paying, as penalty for some injury against Rome), or simply attached to their provincial administration. City government basically followed the pattern of the city Rome, that is, an executive of *duoviri*, a curia of leading citizens, and a popular assembly. But within these guidelines there were variations of detail deriving from geographical and cultural differences among the peoples of the empire and the sheer multiplicity of towns and their products; and indeed during Stahl's centuries the status of many cities changed, and numerous new *municipia* of all Roman types were created, including Latin and Roman colonies, with their respective degrees of citizenship, some few with the *jus Italicum*, or tax immunity. The trend, however, was always to remove immunity, and "citizenship was a gift conferred in proportion to its valuelessness."

To surmount this immense variety of cases, Stahl is concerned with developing some historical model for studying social change. He concludes that recent theories of social mobility do not easily transfer to the Roman social system, because ancient sources limit our vision to an upper class mobility and force us *e silentio* to an awareness that the vastly more populous lower strata had little mobility.

A sizable section is devoted to the evolution of the *ordo decurionum* or town senate, its privileges and increasingly compulsory system of liturgies or public services.

The most valuable portion of this study deals at length with the scope of municipal activities (corn supply, games, public buildings, and the like) and

the *Interventionspolitik* that finally ended their autonomy and effected the intrusion of Roman *curatores/correctores*, usually for financial shortcomings, real or imputed (pp. 73–136).

This book is an engaging composite of general theory and specific examples, some little known. Stahl's bibliography will direct the reader expertly over the field.

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ROBERT M. GRANT. *Eusebius as Church Historian*. Oxford: Clarendon Press of Oxford University Press. 1980. Pp. 184. \$29.95.

Robert M. Grant, the *doyen* of ancient church historians has brought together the results of his earlier studies on Eusebius in this important volume. He believes that "The only way to understand the *Church History* is to view it as a process, not a finished achievement" (p. 10). He argues that alterations may be detected not only in books 8 through 10 but also in the earlier sections of Eusebius' *magnum opus*.

In cases where Grant can compare Eusebius's earlier *Chronicle* with the *Church History*, or his five different works dealing with martyrs, or the same passage in different manuscripts, the evidence for changes is unassailable. As examples of such changes between the "first" and the "second" edition, Grant notes that Eusebius placed the death of James, the brother of Jesus, in 61, but then under the influence of Hegesippus incorrectly shifted it to before the siege of Jerusalem. He seems to depict his hero, Origen, at first as a martyr, and later as a non-martyr.

More problematic are cases where such contradictions lie embedded in the same manuscript, such as Grant's analysis of Eusebius' tergiversations on the authorship of the Apocalypse. It is clear that he accepted at first the apostle John as the author (in the *Chronicles*), and that in the end he rejected his authorship. By reading between the lines, Grant also argues that between these stages Eusebius not only cited but also agreed with the opinion of Gaius that the Apocalypse was written by the Gnostic heretic Cerinthus (pp. 126–36). If this had indeed been the case, was Eusebius so dull as to be unaware of such a glaring contradiction? On the other hand, as he was making other corrections, why did he not expunge this misleading passage also?

Grant cites with approbation the views of T. D. Barnes (p. 14) on the editions of the *Church History* without much elaboration. Barnes's own detailed arguments that there may have been four editions of the *Church History* (in ca. 295, ca. 313–14, ca. 315,

325) are now set forth in "The Editions of Eusebius's *Ecclesiastical History*," (*Greek, Roman, and Byzantine Studies*, 21 [1980]: 191-201).

In a volume of such slender size covering broad topics it is inevitable that many issues are mentioned summarily or not at all. Five examples including the following: (1) In discussing Josephus's testimonium to Jesus (pp. 100-01) Grant accepts C. Martin's suggestion that the Christian interpolations were Origen's marginal notes. But Eusebius, who would have known the difference between the writings of Josephus and Origen, would then be guilty of consciously foisting upon his readers glosses as part of the original text of Josephus. One misses any reference to S. Pines's discovery of an Arabic manuscript of Josephus. (2) Grant devotes less than a page to Simon Magus and Menander (p. 87), and concludes that Eusebius' accounts "possess no value apart from that of the documents he quoted or paraphrased" (p. 86). Though he cites some of the Nag Hammadi texts, readers would hardly guess from this book that the age and origin of Gnosticism is one of the most debated issues of our day. (3) He refers to the tradition that Christians escaped from the fall of Jerusalem to Pella (pp. 48, 70, 108) without discussing the authenticity of the tradition and with no mention of the bearings of excavations at Pella. (4) Grant notes that Eusebius may have cited from imperial decrees to brand Manichaeism a Christian heresy. He does not mention the sensational discovery of the Cologne Codex on Mani's life. (5) In view of Eusebius' aim to demonstrate that "orthodoxy always preceded heresy," it is disappointing to find no discussion of W. Bauer's provocative thesis that the reverse development was frequently true.

In summary, admirers of Grant's prolific scholarship will be grateful for this lucid and stimulating exposition of Eusebius but must await other publications for his judgment on these issues.

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## MEDIEVAL

PETER BROWN. *The Cult of the Saints: Its Rise and Function in Latin Christianity*. (Haskell Lectures on History of Religions, new series, number 2.) Chicago: University of Chicago Press. 1981. Pp. xv, 187.

It is hard to read this brief but elegant and original book, the 1978 Haskell Lectures at the University of Chicago augmented by fifty pages of notes, and not be attracted to the author's major propositions. These maintain that the cult of the saints implied a radical departure from practices and attitudes of classical society, attracted and preoccupied the no-

blest minds of the church, possessed an indisputable centrality in the religion of late antiquity, and served as a vehicle of social integration and cohesion in the urban communities of the late Roman world.

The worship of Christian saints placed the dead into places and contexts from which they formerly had been rigidly excluded and turned the remains of humans from objects of horror into focal points of divine power whose proximity the believer could not help but seek. This inversion of earlier perceptions is rightly emphasized here. Turning against a tradition that has been dominant among writers from the Enlightenment until now, Peter Brown rejects the idea that the rise of the Christian saints' cult resulted from the unchanging adherence of the superstitious masses to a primitive religious polytheism and takes note of the essential participation of the Christian elite in creating the heroes of the new faith and developing their sanctuaries. Brown argues that the debates over the worship of saints in the late fourth century were not between a populace craving the white magic of the saints and a spiritual aristocracy immune to such infections but between the wealthy family, with its penchant to "privatize" religious practice and the total Christian community represented by the bishop. It was a question of which earthly guardian would control access to the heavenly patron. The bishop's victory in this battle over the families of his own class became both an instrument and a symbol of the entrenchment of episcopal power in the West. In a book rich in acute observations and bold generalizations, this is an original and persuasive interpretation.

The relationship between saint and cultist replicated the bonds of patronage and friendship existing in late antique society. The saint became the patron, protector, friend, and intimate, with whom the worshipper could communicate, even identify. Although it may be the rule that a society perceives its saints in the image of its personal and social relations, Brown also makes the point that this projection of the real into the supernatural made it possible to raise questions about power and dependence in the contemporary world. Brown views the translations of relics as means of creating good will and solidarity and therefore as indicators of ties of patronage and alliance that bound together the clerical and lay elites of the late Roman world. They also served as occasions for the integration into the urban community of groups formerly excluded, especially women and the poor, and demonstrations in the proper exercise of power, since the saint had suffered from the evil exercise of power. Brown is well aware that the cult of the saints defined itself rather narrowly in terms of the human relations of an urban and aristocratic society, but

rarely has an author used with equal success the literary, liturgical, and architectural manifestations of this cult, often so frustrating in their conventionality, to probe the relations of power in and the pre-occupations of a society.

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ROBERT DELORT. *Le commerce des fourrures en Occident à la fin du moyen âge (vers 1300-vers 1450)*. In two volumes. (Bibliothèque des Écoles Françaises d'Athènes et de Rome, number 236.) Rome: École Française de Rome. 1978. Pp. lxvi, 560; 564-1,383. 475 fr.

This handsomely produced study of the late medieval fur trade is Robert Delort's revised doctoral dissertation defended in 1975 at the University of Paris IV. True to the genre, it is grand in conception, crowded in detail, and immense in length. The work is divided into four main sections prefaced by an impressive catalogue of the manuscript sources consulted, an exhaustive list of published sources, and an extensive bibliography of secondary material. Delort seemingly enjoyed the time and resources to visit virtually all appropriate European archives—twenty-one in Italy alone—including those of Russia, Poland, and Yugoslavia.

In the first part the author traces the various types of furs circulating in the international and local European markets and the geographical and climatic range of the animals from whose bodies they came. It would appear that anything on four legs, from bear to rabbit, wildcat to house cat, was likely to lose its skin to the voracious European appetite for furs. Animals were chased, trapped, and raised domestically to supply the outsized demand. Fur-bearing animals are classified and described here according both to their natural habitat—water (otter, muskrat, beaver, mink), forest (lynx, wolf, fox, weasel, marten, some 300 varieties of squirrel, and rabbit), mountain (bear, antelope, wildcat), or barnyard and pasture (rabbit, cat, goat, sheep)—and to their region (northwest, Mediterranean) or country of origin (Russia, Poland, Scandinavia).

Part 2 is an analysis of the factors that influenced the demand for furs. Although furs were employed extensively as blankets, pillows, wall coverings, and upholstery, Delort points out that the history of their use is essentially a chapter in the history of fashion. He singles out three main considerations that conditioned patterns of consumption: climate and the need for protection against the cold, modesty and the desire to cover the body, and the urge to decorate the body. As to the latter, from around 1340 the tendency in style was to lighter, tight-fitting garments that required less fur. The resulting

drop in aggregate demand was offset, however, by another change in fashion, the adoption between ca. 1360 and ca. 1425 of a long, furred, capelike outer robe, the *houppelande*, as standard dress by both males and females of the aristocracy. A *houppelande* "trailing to the ground" belonging to the duke of Burgundy in 1402, for example, consumed the skins of 500 martens. Two royal robes mentioned in 1414 employed 540 and 545 sable skins respectively. The trend setters of these and other modes of fashion, argues Delort, were the French royal court in the fourteenth century and the court of Burgundy in the fifteenth. The latter half of the fifteenth century witnessed the emergence of greater diversity of dress expressed in "national" styles.

The third section is an examination of the markets for furs and a description of how furs were delivered to those markets. Delort considers this commerce from the perspective of seignorial courts (*milieu palatin*) and the towns (*milieu urbain*). Kings, princes (lay and ecclesiastical), and feudal lords were generally prime consumers of fur. Kings and princes kept considerable quantities of furs "in stock" in their treasuries or wardrobes against which they could draw as circumstances demanded. Not only were huge quantities of furs used to clothe kings, princes, and their families—in a five-month period in 1335, the nine princes of the French crown consumed furs valued at £ 6,464 9s., while the king himself used £ 2,056 11s. 4d. worth—but furs were distributed to retainers and members of the court in livery, given to visitors at court as gifts, and disbursed among the neighboring populace as largesse.

Cities in the west were both markets for as well as centers of finishing and distribution of furs. Delort identifies not only the urban patricians but also master craftsmen, artisans, nobles residing in the cities, urban clerics, and monks in addition to the nobility, clergy, and artisans of the surrounding countryside as consumers. The principal purveyors of furs to the urban market were the *pelletiers* who finished the skins and also engaged in retail and wholesale commerce. In economic terms, the majority of *marchand-pelletiers* were moderately well off, although some of them achieved considerable wealth and prominence within their cities.

The final major section of the book is a further consideration of the urban commerce in furs. The *pelletiers* sold furs on the retail market, exchanged them among themselves, and journeyed to nearby—never distant—fairs and towns to dispose of their goods. The author examines the structure of the movement of fur in international and regional commerce, concluding with an analysis of the prices of various furs and a calculation of the profits derived by the merchants carrying them over long distances.



A general summary and indexes of authors cited, place names, personal names, and French and foreign terms close out this remarkable study. It should also be added that the accompanying tables, charts, and graphs are unusually clear.

The European commerce in fur in the late Middle Ages was a complex business. It is impossible in the course of a brief review to convey the breadth and depth of this book. Delort covers not only the commerce in furs but every conceivable factor that influenced that traffic. His work should appeal not only to economic and social historians but also to anyone with a general interest in the later Middle Ages.

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ALFRED P. SMYTH. *Scandinavian York and Dublin: The History and Archaeology of Two Related Viking Kingdoms*. Volume 2. Atlantic Highlands, N.J.: Humanities Press or Templekieran Press, Dublin. 1979. Pp. x, 361. \$15.00.

This work completes a two-volume study of the Anglo-Scandinavian city of York and the Hiberno-Scandinavian port of Dublin, of which the first part was published in Ireland in 1975. It can indeed be read independently as an account of the years from 921 to 954, just as the earlier volume covered the period beginning 857. But, because the chronological framework of both volumes is based on Alfred P. Smyth's interpretation of the dynastic history of the kings who ruled from York and Dublin, it is difficult to follow the details critically without recourse to Smyth's *Scandinavian Kings in the British Isles* (1978).

Volume 2 begins with the conflict of the grandsons of Ivar "the boneless" with Athelstan of Wessex up to the overthrow of Gothfrith's grand alliance of Vikings, Scots, and others at Brunanburh in 934. There follows an analysis of the source material for the battle, its possible location, and its wider political context. The reversal of the outcome of Brunanburh by Gothfrith's son Olaf and the subsequent history of the Scandinavian rulers of York are then traced through to the expulsion and slaying of Eric "Bloodaxe" in 954. Archaeological evidence for urban economy, topography, and material culture is described in two substantial chapters. Other sections are devoted to the Scandinavian treatment of monasteries, especially in Ireland; to the witness of carved crosses and gravestones for the spread of Christianity among the pagan Scandinavians, and to rather thinly drawn parallels between Dublin and York on the one hand and the great trading cities of Russia on the other.

Smyth recruits his evidence from an extraordi-

narily wide range of sources: annals, sagas, and chronicles of Irish, Scandinavian, British, Pict-Scottish, and Anglo-Saxon origin. But their very range must provoke a degree of skepticism about the details of both narrative and interpretation. This is especially so when we find Smyth giving weight to later writers such as William of Malmesbury or Snorri Sturluson, without establishing the sources that they used. To this reviewer, moreover, Smyth's chains of reasoning often appear to be poorly fashioned. A clear example of this is in the discussion of the location of Brunanburh, where he makes the sweeping generalization that a conflict involving so many people is unlikely to have occurred in a remote Cumbrian dale. This is unverifiable in itself, and the earlier example of Degsaetan may suggest that it is not even sound. But Smyth uses it to buttress his own location for the battle, about which other scholars have preferred to say *nescio*.

Despite such reservations, the author may be congratulated on his breadth of vision and on the new interpretations to which this has led him.

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KATHLEEN HUGHES. *Celtic Britain in the Early Middle Ages: Studies in Scottish and Welsh Sources*. Edited by DAVID DUMVILLE. (Studies in Celtic History, number 2.) Woodbridge, Suffolk: Boydell Press or Rowman and Littlefield, Totowa, N.J. 1980. Pp. ix, 123. \$35.00.

This book by Kathleen Hughes consists of six studies in Scottish and Welsh primary sources. Each essay presents fresh and penetrating insights into particularly knotty problems, and each carries with it far-ranging implications. Three of the studies have been previously published. "Early Christianity in Pictland" suggests that Bede's account of the evangelization carried on by Ninian and Columba may be overdrawn. "British Library MS. Cotton Vespasian A. xiv (*Vitae Sanctorum Wallensium*): Its Purpose and Provenance" argues that the manuscript was intended as a historical, not a liturgical, document and that its provenance was originally Gloucester, not Monmouth or Llandaff. In "The Welsh Latin Chronicles: *Annales Cambriae* and Related Texts" Hughes sorts out the tangled stages in the development of the *Annales*, including a reconsideration of the dating of its original version. If Hughes is right—and I believe she is—in assigning a date of the late eighth century, then Leslie Alcock's argument in his *Arthur's Britain* (1971) that "the fifth- and sixth-century items in the British Easter Annals could have been contemporary entries in an Easter Table" (p. 49) is incorrect. This in turn suggests



that the Annals are *not* a reliable source for the authentication of Arthur as a historical person—at least not the Arthur of heroic legend.

Apart from these published studies, three essays are presented here for the first time. "Where Are the Writings of Early Scotland?" considers the provocative problem of why we possess so few written sources, especially chronicles and king-lists, for early Scottish history. After dispelling the usual interpretations that the "ancient histories were destroyed or carried off in King Edward I's time" (p. 3) or that Reformation mobs, hell-bent to destroy "popish stuff," were responsible, Hughes concludes that "there had been little written history and written literature in early Scotland, and that history and literature were mainly oral" (p. 17).

"The Book of Deer (Cambridge University Library MS. Ii.6.32)" transcends earlier studies in struggling *inter alia* with questions of dating and provenance of the original Book of Deer. For example, whereas Kenneth Jackson, in his *Gaelic Notes in the Book of Deer* (1972), described Deer as "apparently of the ninth century" (p. 8) and threw in the sponge concerning its provenance, arguing that "there is nothing to show where it was written, whether in Ireland or Scotland, whether at Deer itself or somewhere else within the Gaelic Christian world" (p. 9), Hughes relegates Deer to "the first half of the tenth century" (p. 25), and she concludes that it "was written in some provincial scriptorium, quite possibly in Scotland" (p. 37). The same methodology is applied in "The A-Text of *Annales Cambriae*," a study that expands upon Hughes's earlier published article on the *Annales*. In answer to the questions "When do the *Annales Cambriae* become contemporary and where were they drawn up?" (p. 86), Hughes observes that "the *Annales Cambriae* show signs of specific localisation for the first time at the end of the eighth century" (p. 100) and that they were begun at St. David's.

GEORGE B. STOW  
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JEFFREY H. DENTON. *Robert Winchelsey and the Crown, 1294–1313: A Study in the Defence of Ecclesiastical Liberty*. (Cambridge Studies in Medieval Life and Thought, series 3, number 14.) New York: Cambridge University Press. 1980. Pp. x, 341. \$42.50.

The name of Archbishop Winchelsey appears regularly in articles and monographs on this important period in the history of medieval England, and that prelate's role in the *Confirmatio Cartarum* forced upon Edward I and in the ordaining movement under Edward II is well established. While Jeffrey H. Denton touches upon these and related developments, his principal concern is Winchelsey's pro-

longed contest with the crown in the defense of the rights of the English church in the matter of clerical taxation. The wars of Edward I, the troubles of Edward II, and the fact that Pope Boniface's *Clericis Laicos* set off more dramatic reverberations in France than in England have kept this aspect of Winchelsey's accomplishment under something of a shadow. Denton brings it in full view and introduces so dedicated, independent, and uncompromising a personality to the reader that one sees why neither crown nor papacy wanted any more like him.

Given his family's lack of distinction, Winchelsey's abrupt rise from chancellor of Oxford to archdeacon of Essex and then to the primacy attests to his reputation as a scholar, administrator, and leader. This last quality—the English medieval church never had a stronger leader than Winchelsey unless it was Becket, to whom he has been likened—would have made Edward I wary of accepting his election as archbishop had he foreseen the wars with the Scots and with France and the financial pressures these would entail. To meet the expenses of these wars he would require large subsidies from the church and the cooperation of a passive prelate in Canterbury. In Winchelsey he found a stone wall.

It is to these difficulties over clerical subsidies between Winchelsey and Edward that Denton gives most attention. The study does take time to reveal Winchelsey as the zealous metropolitan deeply concerned about his first responsibility, that of maintaining the spiritual health of the church under his jurisdiction. The visitations of the dioceses that he undertook were thorough and as extensive as time and health would permit. He did reveal a measure of judiciousness, a virtue his critics must have charged him with lacking, in bypassing the dioceses of those of his suffragans whose first loyalty was to the king: no point in becoming involved in fruitless contests with the crown over matters of lesser importance.

When a major issue such as the taxation of the church's spiritualities was in dispute, there was no flinching. Where the French hierarchy sided with Philip in blocking Boniface's *Clericis Laicos*, Winchelsey ordered it published. He refused to buckle under when Edward outlawed the clergy, indeed kept up the fight even after Boniface had capitulated and had granted the king the right to decide for himself when enough of an emergency existed to warrant taxing the clergy. Edward finally had to bend to Winchelsey and accept the right of the English church to determine for itself when such an emergency existed. In the end, ironically, although Winchelsey won his battle with the crown, he lost to Clement V. To gain Edward's support for a crusade, Clement himself taxed the spiritualities of the

English church and turned the revenues over to Edward! Against the pope Winchelsey was helpless.

An appendix showing royal taxation of the clergy during the period of Winchelsey's primacy, a bibliography, and a general index round off this scholarly monograph.

JOSEPH H. DAHMUS  
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BERTRAM RESMINI. *Das Arelat im Kräftefeld der französischen, englischen und angiovinischen Politik nach 1250 und das Einwirken Rudolfs von Habsburg*. (Kölner Historische Abhandlungen, number 25.) Cologne: Böhlau Verlag. 1980. Pp. viii, 377. DM 88.

In his lengthy introductory section Bertram Resmini moves from a survey of the political background of the former kingdom of Burgundy, over which no emperor since Frederick Barbarossa had effectively exercised control, to the establishment of new areas of power (*Kräftefelder*) there. The major portion of the book depicts Rudolf's politics in the Arelate. With practically no power bases either there or in Germany or Italy, he followed his inauspicious beginnings by an attempted marriage alliance with England, then by a coalition with the house of Anjou. After 1282 the emperor, relying essentially on what support he could muster in the kingdom of Arles, struggled with imperial Burgundy and the house of Savoy, a policy that culminated in 1291 with the creation of a promising league against Savoy. The league was basically the personal creation of a member of the house of Habsburg, however, and the election of Adolf of Nassau shattered forever any real hope of re-establishment of imperial rule in the Arelate.

The book is extremely dense and difficult to follow; were it not for a series of genealogical tables and excellent maps, the work would be nearly impossible to understand. That is not really the author's fault, for he devotes over 300 pages to a subject that the *Cambridge Medieval History* treats in one and one-half pages. It is simply that the political geography of the Arelate, complex almost beyond belief, is characterized by a *Kleinstaaterei* every bit as bad as that in southwestern Germany; coupled with this is the internationality of holdings in the region. The resulting intricacy makes it difficult to redress the major shortcoming of the study, which does not define the territoriality and boundaries of France and the empire. Many historians doubt that these boundaries were geographically definable but think that they must be conceived of in terms of rights, the only reasonable view in light of the complexities of the Franco-imperial border. The book does not concern itself therefore to any considerable extent with territorial rivalry between Rudolf and the

French kings (two concluding excursuses show that one could not yet speak of a French policy of expansion to the east). What does emerge clearly from the study is a recognition of the impossibility of re-creating the old kingdom of Burgundy: Rudolf's policy in the Arelate may have stemmed from his role as emperor, but he had to act much like any other territorial lord.

Resmini's study is nicely produced (although there are one or more missing lines at the bottom of pages 167 and 285), and it contains much useful information. There is material on bishops and major families in the region, an impressive bibliography, and an astounding apparatus in the footnotes. It is a purely political history of the type that has hardly been produced in Anglo-Saxon countries for over four decades; even though many will find it rather old-fashioned, it does belong in every university library.

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LUDWIG VONES. *Die "Historia Compostellana" und die Kirchenpolitik des nordwestspanischen Raumes, 1070-1130: Ein Beitrag zur Geschichte der Beziehungen zwischen Spanien und dem Papsttum zu Beginn des 12. Jahrhunderts*. (Kölner Historische Abhandlungen, number 29.) Cologne: Böhlau Verlag. 1980. Pp. xii, 628. DM 98.

This fine study is the latest fruit of a renewed interest among German scholars in the history of the Iberian churches during the central Middle Ages. The themes of Ludwig Vones's book are the ecclesiastical politics of the northwestern quarter of the Iberian peninsula between 1070 and 1130 and the relations between its churches and the papal curia. It would be difficult to praise his work too highly: it is learned, comprehensive, critical, balanced, and orderly—as nearly definitive as the surviving evidence permits such a study to be.

The principal source of information about these subjects is the *Historia Compostellana*, commissioned by Diego Gelmírez, bishop and subsequently archbishop of Santiago de Compostela. Vones's first two chapters deal with the *Historia* itself, its historiographical context, manuscripts, structure, and authorship. Chapter 3 surveys the period between 1095 and 1120 with special reference to relations between the bishopric of Compostela and the metropolitan see of Braga. Chapter 4 concentrates on the prolonged negotiations that resulted in the raising of the see of Compostela to archiepiscopal rank and the ensuing conflicts between Diego and his foremost rival, Archbishop Bernardo of Toledo. Chapter 5 examines the years between 1124 and 1130.

The general lines of our understanding of Span-

ish church politics are not affected by Vones's work. His vital contribution has been, through close analysis of the sources, to fill out the detail of the picture more sharply than ever before and to relate Diego's public career both to the secular politics of León-Castile-Portugal and to changing papal policies during the period. Two criticisms may be ventured. The lesser is that the detailed analysis is not always convincing. For example, Vones provides an elaborate discussion of the election and consecration of Diego (pp. 100-74). It is a heroic attempt to make sense of these mysterious episodes in his career, but in the last resort it is perhaps a little too ingenious to be altogether persuasive. This is partly because our sources are meager, definitive conclusions rarely possible. But it also arises from Vones's view of our principal witness, the *Historia Compostellana*. This is the second focus for doubt. Much of his argument is underpinned by the conviction that the *Historia* was a carefully wrought product of art, its omissions and apparent confusions to be explained by its authors' aim, which was to persuade. Of course this is true up to a point. But a slightly different emphasis might be proposed. If we ask ourselves how the *Historia* was composed we might be better placed to answer the question why it was composed. To my mind the internal evidence of the text suggests that composition was less coherent, more haphazard, than Vones and others have suggested. Lack of consistency in composition makes it hard to believe in consistency of purpose behind the text as we have it. Despite Vones's admirable work, I still think the authors of the *Historia* were more muddled than cunning.

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HERBERT HUNGER. *Die hochsprachliche profane Literatur der Byzantiner*. Volume 1, *Philosophie, Rhetorik, Epistolographie, Geschichtsschreibung, Geographie*; volume 2, *Philologie, Profandichtung, Musik, Mathematik und Astronomie, Naturwissenschaften, Medizin, Kriegswissenschaft, Rechtsliteratur*. Assisted by CHRISTIAN HANNICK and PETER E. PIELER. (Handbuch der Altertumswissenschaft, series 12, Byzantinisches Handbuch, number 4.) Munich: Verlag C. H. Beck. 1978. Pp. xxvi, 542; xx, 528. DM 138; DM 148.

When Karl Krumbacher published his *Geschichte der byzantinischen Literatur* in 1891 as part of Iwan von Mueller's *Handbuch der klassischen Altertumswissenschaft*, the greatest German classical scholar of the age, Ulrich von Wilamowitz-Moellendorff, enthusiastically declared that Krumbacher had almost created a new science. He meant by this that the study of Byzantine literature was of such complexity and importance that it should be regarded as a field of study and research outside the tradi-

tional confines of classical scholarship. Six years later a second, greatly enlarged edition appeared that included a discussion of Byzantine theological literature by Albert Ehrhard and a brief survey of Byzantine history by Heinrich Gelzer. It is no exaggeration to say that Krumbacher's history of Byzantine literature was the most important contribution to Byzantine studies since the pioneering work of DuCange in the seventeenth century.

Although it remains an indispensable guide for students of all aspects and periods of Byzantine history and literature, hundreds of unpublished texts have been edited, new critical editions of most of the major Byzantine writers have been published, and commentaries and critical studies of all kinds have appeared since the publication of Krumbacher's study nearly a century ago. A new history of Byzantine literature was clearly needed. Progress has been such that the writing of a new handbook was thought to be beyond the expertise of a single scholar, and the task of revising Krumbacher was entrusted to two of the world's most prominent Byzantinists, Hans-Georg Beck and Herbert Hunger. Beck's *Kirche und theologische Literatur im byzantinischen Reich* appeared in 1959, followed in 1971 by his *Geschichte der byzantinischen Volksliteratur*, replacing the sections of Krumbacher that dealt with theological and popular literature respectively. The work of revision has now been completed with the publication of Hunger's two volumes, *Die hochsprachliche profane Literatur der Byzantiner*.

Byzantine literature can be divided roughly into two classes. The two classes are differentiated by the level of the language used by the writer, an archaic or Atticizing Greek in formal writing and a mixture of vernacular and biblical *koine* forms in writing of a more popular nature. The choice of language was increasingly dictated by the audience for which the particular works were intended. Works written in the *Hochsprache* or Atticizing language were accessible to a diminishing audience of educated clergymen and civil servants as the gulf between the spoken language and the ideal language of the Atticists widened. Literature in the Byzantine *koine* was usually of a religious or utilitarian content and was directed primarily toward the lower orders of the clergy and the semiliterate monks. Beck's two volumes discuss about one-third of the works covered by Krumbacher-Ehrhard. The remaining two-thirds of the literature that survives from the Byzantine period is nontheological and is written in the artificially archaic *Hochsprache* and in a very elaborate rhetorical style.

Hunger has arranged his history of Byzantine secular literature by genre, not chronologically or by individual authors. The first volume discusses philosophy, rhetoric, epistolography, historiography, and geography. The second volume deals

with the study of classical philology in Byzantium, secular poetry, music (by Christian Hannick), mathematics and astronomy, the natural sciences, medicine, military science, and legal writings (by Peter E. Pieler). Each section contains a discussion of the principal characteristics of the genre and provides short summaries of the most representative works. This arrangement makes the volumes difficult for the nonspecialist reader to consult. The reader must refer to the index and skip around the volumes in order to follow the career of even the most important Byzantine writers like Michael Psellos, Tzetzes, or Eustathios. This is a weakness inherent in the decision to discuss Byzantine literature by genre. Many of the most accomplished Byzantine writers could be most accurately described as polymaths, and this is often obscured by the separation of an author's works throughout several sections in the two volumes.

The bibliographies that follow each section are arranged by date of publication, not by subject matter. Again, this arrangement often makes it very awkward to survey the bibliography on any particular author or work. This difficulty is overcome in some instances by the footnotes to the text, which refer to the relevant bibliography for a specific work. In the section on historiography, Hunger refers the reader to the bibliographies compiled by M. E. Colonna, *Gli storici bizantini dal IV al XV secolo*, volume 1, *Storici profani* (1956), and G. Moravcsik, *Byzantinoturcica*, volume 1, *Die byzantinischen Quellen der Geschichte der Türkvölker* (1958), and lists only works that have appeared since the publication of these two reference books. Unfortunately neither of these books is commonly found in American university libraries.

Although Hunger's volumes are somewhat inconvenient to consult, his impeccable scholarship and the vast range of his knowledge of Byzantine literature make his contribution to the revision of Krumbacher a monument of modern Byzantine scholarship somewhat analogous to the volume he has carefully revised and brought up to date. His work does not entirely replace Krumbacher, who will still be consulted for the magnificent achievements of nineteenth-century German scholarship, nor does it make the publication in the near future of another history of Byzantine literature arranged by author any less a desideratum.

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#### MODERN EUROPE

D. P. WALKER. *Unclean Spirits: Possession and Exorcism in France and England in the Late Sixteenth and Early Seven-*

*teenth Centuries*. Philadelphia: University of Pennsylvania Press. 1981. Pp. 116. \$15.00.

This is a wonderful little book: an introduction to a topic vulgarized by popularizers and neglected by specialists. Although D. P. Walker is modest in his goals—the “small step” into an unexplored field—the ramifications of his analysis are broad. Not only does this book shed light on the topics of possession and exorcism, but also larger subjects such as witchcraft, magic, and the religiously oriented society of the early modern period are illuminated as well. Walker further validates Mandrou's thesis that the connection between possession and witchcraft in the seventeenth century was a major cause for the decline in the successful prosecution of witchcraft. Whereas a jury could not observe a witch practicing her evil craft, the demoniac's fits could be carefully watched, examined, and invalidated.

Walker titillates the reader with new questions and new suggestions. Why were such a large number of women possessed? Was it an opportunity for women in a male-dominated age to preach from a pulpit? What were the ramifications of the Puritans' disdain for miracles? Not only does Walker suggest that this belief in the cessation of miracles after the apostolic age caused the Puritans to look askance at possession, but also he goes further, stating that a world operating without miracles, “through normal, natural means,” was favorable to the development of early modern science.

Walker's thesis is that the phenomenon of possession can be understood as a combination of disease and fraud. A person (usually a girl) began by being genuinely sick; possession was suggested, and she became both sick and fraudulent. Through a few case studies, Walker demonstrates how this syndrome operated. Yet his theory, although illuminating, is incomplete. Even though Walker is not against psychological explanations, his emphasis is wholly on the “rational” basis of possession. But by excluding the presence of supernatural phenomena, he also excludes the inner devils of psychological malaise. Walker notes that the diseases associated with possession are epilepsy, hysteria, and melancholy, diseases he believes to be physically induced. But are they? All three have strong psycho-pathological implications. Could it be that the answer to possession lies not in the combination of fraud and sickness but in the unconscious demons of psychological pathology?

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FELICITY HEAL. *Of Prelates and Princes: A Study of the Economic and Social Position of the Tudor Episcopate*. New



York: Cambridge University Press. 1980. Pp. xvi, 363. \$39.50.

The retaining by the post-Reformation Anglican Church of its order of bishops has been a fact of the utmost significance in world history, not least in current efforts to reunite the Christian churches. It is therefore timely that an attempt should be made to follow the fortunes and define the role of the bishops of Tudor England. Felicity Heal is one of a group of younger scholars who are rewriting the history of the Tudor church at a local level, resting their findings firmly on the rich diocesan archives. Fortified by her own work on the diocese of Ely, Heal has extended her inquiries in depth over a few other dioceses and in a number of respects has managed to comprehend every one of twenty-four. She compares episcopal income in 1535 and 1553 and provides maps showing the location of episcopal manors in 1535 and 1603. Administrative practices and the opening up of doctrinal gulfs are never lost sight of, but Heal's main concern is with episcopal resources and with the bishops' place in contemporary society.

For the first time the exact chronology of the depletion of bishops' lands is made clear, beginning with Henry VIII's exchanges of land with Archbishop Cranmer. The crown, predictably, emerges as the controlling agency. The direct diversion of resources into the hands of the laity began, we learn, with the granting of long leases, and indeed outright grants came only in the reign of Edward VI. Although unable to discover details either about the exaction of entry fines for the leases or about any "consideration" in respect of outright grants, Heal does wonder how else John Voysey, bishop of Exeter, managed to finance his excellent benefactions for the people of his native Sutton Coldfield.

Heal goes far to win the sympathies of her readers for these often too much maligned prelates. Before the Reformation they were, however humbly born, the equals in both wealth and standing of the aristocracy. By the later sixteenth century, although still expected to maintain large households, dispense lavish hospitality, and contribute substantially to public service and finance, they were really only the equivalent economically of the wealthier country gentry. Heal is able to show that, despite the queen's disapproval, most of the Elizabethan bishops were married and on average had four children, in both respects taking greater advantage of their opportunities than the lower clergy. She exculpates them of unprecedented exploitation of their official property: even pre-Edwardian bishops had had relatives.

As indicated by her title, Heal sees the crown as master but in the long run protector, retaining the bishops because they were useful, perhaps on ac-

count of their wide diversity of talents and opinions. Altogether this is a very well structured book and one that is a delight to read.

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J. A. GUY. *The Public Career of Sir Thomas More*. New Haven: Yale University Press. 1980. Pp. xii, 220. \$22.50.

As incredible as it may be, scholars have waited a mere 346 years for the appearance of the first book-length study of Thomas More's political career based on archival research. It is easy and convenient to forget that the author of *Utopia* was politically ambitious, that he competed in the area of politics with the duke of Norfolk and Thomas Cromwell, and that he was a persecutor of Protestant heretics. J. A. Guy's important book examines More's career from three perspectives: his work as councilor to Henry VIII, his work in the courts, and his role in politics from 1529 to 1532.

The son of a successful, self-made justice of the King's Bench, Thomas More had the proper pedigree and education for political success. Guy rejects Erasmus's notion that More strove to avoid service at court because he preferred a life of scholarship. He joined the king's council in 1517 and four years later received the valuable appointment of under-treasurer of the Exchequer. Working as a resident humanist, royal secretary, and diplomat, More won the affection of the king and Cardinal Wolsey. Yet Guy finds that More had little influence over the affairs of state before 1529.

The fall of Wolsey led to More's appointment as chancellor, a position that fully utilized his legal talents and experience. Because of inflexibility and inefficiency in the common law courts, the era of Wolsey saw "a mass defection of litigants to the chancellor's courts of Chancery and Star Chamber." As chancellor, More continued Wolsey's judicial policy and handled an increasing number of suits in Chancery. The work of Star Chamber also remained at a high level under More. He attempted but failed to reform deficiencies in the legal system. When More resigned in 1532 after thirty-one months in office, a substantial backlog of litigation remained for his successor. In a meticulously researched section on More's work in the courts, Guy challenges the interpretation developed by More's son-in-law, William Roper, and perpetuated by literary biographers. He reveals a new More, who followed in the footsteps of Wolsey and added a practical dimension to the cardinal's concept of impartial justice.

However much More may have wished to devote himself to the courts, the crisis arising from Henry



VIII's determination to divorce Queen Catherine forced him to take an active part in politics. In 1529 More had no base of power, and he alone could not resist the influence of Wolsey's political heir, the duke of Norfolk. More joined with a group, whom Guy terms the Aragonese faction, that was loyal to Queen Catherine, united against heresy, and determined to defend the church. He and his clerical allies struggled unsuccessfully to resist Norfolk and a group of radicals that included Cromwell and Thomas Audley. In explaining More's political demise, the author attaches great importance to the Common's Supplication against the Ordinaries. When Convocation wavered in the face of the king's support of the Supplication, More resigned as chancellor. "More had played his part and lost." He discharged his duty and "quit the arena in good conscience" knowing that Cromwell's victory was "narrow, blundering and legally suspect" (p. 201).

Guy offers a study that is at once more and less than a complete political biography. He has important things to say about Wolsey and Christopher St. German, and he provides new insights into the complexities of court politics between 1529 and 1532. On the other hand, the book ends in 1532 and gives no account of More's trial and execution three years later. More's career often gets lost among the author's other interests in chapters that suffer from poor organization and rather lengthy paragraphs. Although based on laborious archival research, the book lacks a bibliography. Furthermore, Guy's conclusion that More had a brilliant career in law and politics is at variance with contrary and more convincing arguments that show More to have been a poor second to both Wolsey and Cromwell. Guy's work, however, is of sufficient stature to withstand criticism; it is a major contribution that must be read by all students of More and early Tudor England.

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JANE GARRETT. *The Triumphs of Providence: The Assassination Plot, 1696*. New York: Cambridge University Press. 1980. Pp. xi, 289. \$19.50.

Despite both the enormous contemporary interest aroused by the assassination plot of 1696 and its obvious importance in the political struggle of the 1690s, the plot's origins and inner workings have long escaped thorough examination. Unfortunately, Jane Garrett's study, which provides a detailed account of the events and personalities of the 1696 conspiracy, is marred by several significant flaws. The book is likely to leave scholars frustrated; it contains much that is useful and interesting, yet its defects prompt uneasiness at important points.

The book's value is that it gives a complete narrative of the attempt to assassinate William III at Turnham Green in February 1696. When Louis XIV agreed in December 1695 to invade England on behalf of James II, he made his offer contingent on the English Jacobites rebelling first. But the Jacobites were afraid to rise in advance of a French landing, and a deadlock resulted. To break it, an inner ring of the Jacobite leadership decided to set in motion a plan to kill the prince of Orange, hoping to throw the nation into confusion and prompt both Louis and the Jacobites to act. The plan, of course, failed, and nine of the plotters paid at Tyburn the grim penalties prescribed for traitors.

If the book's strength is in telling a good story, its weakness lies in describing the political and legal context surrounding the story. The notions of court and country, for example, do not appear in the text; the word "Junto" is never used. Thus, the author tells us that the Whigs—as if they were one party—won the election of 1695 (pp. 84, 179); that the assassination plot cemented their position so completely that they won an overwhelming victory in the 1698 election (p. 262); and that as a consequence "King William had an all-powerful Whig government for the rest of his life" (p. 262). Neither the book's failure to distinguish among types of Whigs nor its view of the 1698 election and politics thereafter can be accepted in light of modern scholarship. Similarly, in discussing the law of treason the author concludes that the statute of 1352 was responsible for the unfair trial procedures that, until 1696, burdened defendants in state trials (pp. 178, 206). In fact, the famous act of 1352 says nothing about procedure; it deals entirely with the criminal deeds that constitute treason.

Serious students of the period will be disappointed with the documentation for a number of sections in the book. Many chapters are thinly footnoted, and, worse, the narrative relies on a single, sometimes questionable source at too many points. Moreover, important sources—such as the H. M. C. Kenyon MSS—have been overlooked, and others are incorrectly cited.

*The Triumphs of Providence* may interest general readers, and it will be of use to scholars concerned with Jacobite activities in the mid-1690s. But the book is not for those hoping to understand how such activities fit into the legal and political framework of the period.

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MICHAEL ROBERTS. *British Diplomacy and Swedish Politics, 1758–1773*. (Nordic Series, number 1.) Minne-

apolis: University of Minnesota Press. 1980. Pp. xxv, 528. \$29.50.

Great Britain emerged from the Seven Years' War ready to enjoy the commercial and mercantile gains that it had won. The achievement of this goal was dependent upon the preservation of peace, and, to the Foreign Office, France appeared to be the primary threat to that peace. Michael Roberts's book, *British Diplomacy and Swedish Politics, 1758-1773*, is an analysis of the attitudes and events that guided the British Foreign Office in its formulation of a diplomatic response to the threat posed by the French alliance system, a system that was by no means insignificant. France was bound to Spain by the Family Compact and linked to Austria by alliance. The British, on the other hand, had no permanent allies on the continent. Of the remaining great powers only Russia, anxious to expand its influence in the Baltic and central Europe, had much to gain from a diplomatic association with the British.

British statesmen became convinced that the most desirable way to achieve an Anglo-Russian alliance was by first concluding a treaty with Sweden, a treaty to which Russia might then adhere. This treaty was to be general rather than specific in nature. While an Anglo-Swedish alliance was viewed as a necessary intermediate goal, the British Foreign Office did not want to be burdened with a weak northern partner in the event that an Anglo-Russian alliance never came to fruition.

Roberts's book focuses detailed attention on the period 1758-73, the years during which Sir John Goodricke served as the British minister to Sweden. This was a period when the British government devoted considerable interest and resources to secure an Anglo-Swedish alliance. As the years of the Goodricke mission drew to a close, however, Britain's fear of France began to decline and so, too, did the desire for an Anglo-Swedish alliance. By the spring of 1773, Britain's interest in achieving an Anglo-Swedish, and subsequently an Anglo-Russian, alliance was replaced by a growing interest in an Anglo-French entente.

Although Roberts's account is primarily a diplomatic history, the author has also succeeded in presenting a fine political biography of Sir John Goodricke. In addition, his account provides a detailed examination of Swedish political institutions and the Swedish political process during the last decade and a half of Swedish "liberty" before its overthrow by Gustavus III.

Roberts has made exhaustive use of manuscript sources, including materials at the Public Record Office, the British Museum, the PRO of Northern Ireland, the Bodleian Library, and the Nottingham

University Library. Both public and private archival materials in Copenhagen, Paris, Stockholm, and Uppsala have also been utilized.

All too frequently diplomatic histories tend to become unimaginative chronologies of insignificant encounters and intrigues. This is certainly not the case with Roberts's book. This first volume in the University of Minnesota's six-volume "Nordic Series" is carefully researched and skillfully written. It is a major contribution to both British and Swedish history.

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JOHN BENSON. *British Coalminers in the Nineteenth Century: A Social History*. New York: Holmes and Meier. 1980. Pp. 276. \$42.50.

Recent works by Roy Gregory on miners and politics and by Robert Moore on miners and Methodism, as well as fine area studies by Alan Campbell on Lanarkshire and J. E. Williams on Derbyshire, have considerably expanded our knowledge of British coalminers beyond the standard trade union histories written earlier by R. Page Arnot and others. John Benson is the first, though, to attempt a general social history of coalminers throughout the British Isles during the formative century of the industry.

On the whole, he has succeeded very well. Chapters 1 and 3 analyze the economic structure, regional growth, and differing methods of wage payment in coalfields from South Wales to Yorkshire and from Durham to Ayrshire. More original, and frequently fascinating, are the remaining chapters. Compact, well documented, and filled with vivid quotations, they describe the miner at work, at home, and at play, and also at the task of creating the wide range of cooperatives, friendly societies, and trade unions that provided him with both community support and some degree of financial insurance against old age and the dangers of his daily task. Like Royden Harrison and his co-authors in *The Independent Collier* (1978), Benson challenges the Victorian stereotype of the British miner as thriftless, footloose, and socially irresponsible. He shows that rising real wages, a decline in the size of the family, and improvements in communications in the second half of the nineteenth century enabled many miners to acquire their own homes, set aside money for the future, and establish a relatively stable family life. The spread of branch railways, in particular, made it possible to commute back and forth between mine and colliery town, thereby creating greater social stability and diminishing the presumed isolation of the typical mining commu-

nity. In practice, and again contrary to stereotype, many of these communities were established in or near factory towns such as Barnsley, Wigan, and Newcastle.

Much interesting material comes to light that gives an authentic social portrait. We see the nervous eighteen-year-old collier on his first day as a full-fledged coal hewer, having just graduated from the status of trapper boy or pony driver. Being at his physical peak, he almost immediately made more money than his father. We see the prematurely aging miner's wife, sometimes bringing up six or more children in a three-room company cottage, struggling against dirt, never-ending laundry, and the irregular hours dictated by the multi-shift system to keep a family together. We see the ubiquitous problem of drink, available not simply in pubs but also in illicit stills and shebeens. Toward the end of the century this problem diminished, due not simply to the efforts of temperance societies but also to improved drinking water and alternative sources of amusement devised by the miners themselves.

Yet there are some faults. In his desire to dispose of the conventional portrait of the miner as drunken and socially irresponsible, Benson tends to judge him in terms of middle-class definitions of responsibility without seeing the tension that often existed between the values implied in that definition and the miners' own traditional way of life. For example, Benson comments on the fact that despite the prominent presence of Methodist chapels in the mining towns, large numbers of miners never attended them, but he fails to ask why. He discusses the coming of cinema entertainment without looking at its impact on traditional games and pastimes in terms of the decline of a whole working-class way of life. Above all, he treats the social history of the miners in various parts of Great Britain as a single entity, without taking sufficient account of regional customs and stages of development. This impoverishes, as well as sometimes distorts, Benson's account. Little note is taken, for example, of the impact of Welsh nationalism or of *eisteddfodden* on the culture of the South Wales mining valleys, or of regional dialects, like that reported on by Dave Douglass for Durham. Nevertheless, as a general introduction to a still largely unresearched topic, Benson's book is to be warmly welcomed.

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JAMES TURNER. *Reckoning with the Beast: Animals, Pain, and Humanity in the Victorian Mind*. (Johns Hopkins University Studies in Historical and Political Sci-

ence, 98th series, number 2.) Baltimore: Johns Hopkins University Press. 1980. Pp. xii, 190. \$14.00.

Concern for animal welfare emerged in Britain during the early nineteenth century and spread to the United States after the Civil War. James Turner argues that the Victorian animal protection movement was one reaction of reformers to dramatic changes caused by urbanization, industrialization, the rise of modern science, and the decline of traditional religion.

Based on Turner's 1975 doctoral dissertation, *Reckoning with the Beast* contends that opposition to the abuse of animals often had both moral and practical bases. Bull baiting, for example, resulted in drunkenness and disorder as well as absenteeism and inefficiency; hence both Evangelicals and factory owners opposed such blood sports. At the same time, rapid urbanization fostered nostalgic and romantic sentiments about rural life that caused city people, especially of the middle and upper classes, to move to protect farm animals.

Turner explains that the impact of Darwin and science, especially the notion of the common origins of humans and animals, made reformers aware of animal intelligence and sensitivity to pain. Lurking fear of the nature of man and Victorian anxieties about sex led to demands for the suppression of violent passions. Turner finds, however, that the antivivisection agitation of the late 1800s was little more than a short-lived, hysterical reaction against religious doubt, natural science, and moral flux. By 1900, he concludes, "a wholly new ethic" (p. 125) had emerged, an "ecological ethic: The feeling that, when human beings abused nature, they not merely damaged their own long-term interests but perpetrated a morally vicious act" (p. 127). Conservationism and animal protectionism flourished in subsequent years.

Unfortunately Turner has a proclivity for speculation that results in dubious conclusions. His thesis that morally anxious but economically conservative reformers turned from controversial human problems to an innocuous concern for animals is questionable. Indeed, leading supporters of the Society for the Prevention of Cruelty to Animals, including several mentioned in the text (Wilberforce, Shaftesbury, and the Pease family), were involved in major social, economic, and political reform causes. Even Turner admits: "This sort of displacement of guilt from exploited workers to maltreated brutes would be impossible to document" (p. 54). Nevertheless, he does not hesitate to advance broad generalizations and imaginative explanations on the basis of sketchy evidence.

Turner's essay is a readable, if incomplete, introduction to the subject. Though based on considerable research in primary and secondary sources in

Britain and the United States, it fails to mention such significant supporters of the animal welfare movement as John Bright, who in 1877 denounced "barbarous and cruel treatment of dumb animals." Nevertheless, this brief study is useful background reading for those interested in the current philosophical concerns of Peter Singer and Stephen Clark regarding the moral relationships of humans and animals. The work also provides students of British and American reformism with useful information and provocative observations on one of the more obscure movements of the period.

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A. E. DINGLE. *The Campaign for Prohibition in Victorian England: The United Kingdom Alliance, 1872-1895*. New Brunswick, N.J.: Rutgers University Press, 1980. Pp. 233. \$21.00.

"We are fighting Germany, Austria and Drink; and the greatest of these deadly Foes is Drink," proclaimed David Lloyd George in a speech at Bangor, North Wales, in 1915. His words doubtless raised the spirits of those who had long been engaged fighting the demon drink in the ranks of the United Kingdom Alliance. It is with the organization and political tactics of the Alliance that A. E. Dingle's book is concerned. Founded in 1853, its aim was to persuade Parliament to pass legislation allowing for the suppression of traffic in intoxicating liquor. The vehicle for this was a Permissive Bill that, if given legislative force, would enable ratepayers in any locality to ban the drink trade in their district by vote of a two-thirds majority. In seeking to make use of the power of the state, both at central and local government level, the Alliance proposed a significant change in tactics from the individual, moral suasionist approach of earlier temperance societies.

In the first two decades of its existence, the Alliance conducted its campaign along the lines pioneered by other external pressure groups, most notably the Anti-Corn Law League whose successes the Alliance hoped to emulate. But the failure of the Licensing Bill of 1871 and disappointment over the moderate Licensing Act of 1872 set the organization on a different tack. Instead of seeking to influence MPs and governments of all political colors, it set out to infiltrate one party, the Liberal party.

The bulk of Dingle's book is devoted to a detailed account of this latter phase of the Alliance campaign, with a particular concentration on the years 1871-95 when the organization was at its most active and influential. The exertion of political pressure by a group of organized and dedicated political activists was sufficient to win over a number of Liberal constituency organizations and thus deny elec-

toral support to politicians who did not subscribe to their cause. In the long run, however, their inability to compromise or to moderate their demands ensured the sterility of the campaign and a decline in membership and finances.

At the outset Dingle acknowledges the influence of two previous works, Brian Harrison's *Drink and the Victorians* (1971) and D. A. Hamer's *Liberal Politics in the Age of Gladstone and Rosebery* (1972). As far as the study is concerned, the influence of Hamer appears to have been stronger than that of Harrison. Dingle's book is full of the cut and thrust of political debate, of meetings, elections, and lobbies. For the student of nineteenth-century British politics, it provides a useful addition to the history of pressure groups. For the full story of the role of strong drink in late Victorian and Edwardian society, however, we still await a successor to Brian Harrison's monumental work.

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ANTHONY BRUCE. *The Purchase System in the British Army, 1660-1871*. (Royal Historical Society Studies in History Series, number 20.) London: The Society. 1980. Pp. viii, 194. \$34.30.

Although monographs on various aspects of the purchase system in the British army have been published, Anthony Bruce here attempts to place the subject within a general chronological context. He contends "that neither the broad principles underlying the system nor its detailed functioning have been fully examined" (p. 2).

Bruce traces the evolution of the purchase system from its medieval origins through the Restoration era and into the late eighteenth century. By then, what had been a haphazard practice—by which wealthy officer candidates bought their commissions and advanced in grade by purchasing higher ranks—had developed into a vested property right for the military. Regardless of the blatant injustices of the system, purchase was so deeply entrenched that the aristocracy and landed gentry regarded purchase rights as constitutional principles. Furthermore, rules for prices of commissions, for exchanges of commissions, and for sale of ranks at retirement were so complex that would-be reformers were baffled by the unwritten codes and murky army regulations on the subject.

Yet modifications were underway. In 1720, the prices of commissions were officially published. Under George III, colonelcies and regimental staff appointments were prohibited from being salable commodities. By 1800, Sandhurst graduates could obtain commissions without buying them, and purchase was limited to the infantry, the cavalry, and



the Guards. To control the notorious trafficking in the sale of commissions, the Brokerage Act (1807) prohibited profit making on such transactions. But in practice such restrictions were usually ignored.

In the post-Waterloo period, wealth and family influence were still the dominant factors in obtaining commissions, particularly in the most prestigious regiments. The duke of Wellington defended purchase as essential to keep the army loyal and the nation tranquil. Purchase provided a guarantee, other supporters of the institution claimed, that the officer corps was staffed by members of the same elite.

Even the inefficiency of the army in the Crimean War failed to end purchase. Although parliamentary and periodical critics urged a broadening of the recruitment base for officers, a royal commission in 1857 rejected the abolition of purchase as too drastic a measure. The matter virtually disappeared as a public issue until Sir Charles E. Trevelyan emerged as a vociferous army reformer in 1867. He urged abolition of purchase, higher pay to attract candidates from the middle classes, competitive examinations for appointments, promotion by seniority to the rank of captain, and, above that grade, promotion by merit. Edward Cardwell, secretary of state for war in the first Gladstone ministry, continued Trevelyan's efforts. Bruce follows the intricacies of parliamentary debates, the shaping of public opinion on the question, and the opposition to the reform by the "Colonels" in Commons, by the Lords, and by the duke of Cambridge, the commander in chief. Finally, by royal warrant (July 20, 1871) all regulations related to the purchase, sale, and exchange of army commissions were canceled—a major milestone in the history of the British army.

Yet the social structure of the officer corps barely changed in the following decades, and the abolition of purchase produced little discernible improvement in the military's degree of professionalism. In 1901, for example, only 13 percent of Sandhurst cadets were sons of businessmen, and in 1912 the percentage of titled generals was identical to the figure in 1887. Not until World War I did a real democratization of the army occur.

Bruce has produced a thoughtful and well-organized work that demonstrates a commendable knowledge of the source material. On a subject that has been so thoroughly studied, it is not surprising that he offers little fresh information and few novel interpretations. But in fairness to the author, if he had modest objectives in mind for his volume, he has achieved them. The major flaw in this synthesis is the style of writing. Bruce is so serious about his topic that he overlooks the comic and tragic particulars of purchase. He could have enlivened his book with anecdotes, flashes of humor, and testimonials

about army life from the officers themselves (who very rarely speak in this volume). He has produced a useful book but a dull one.

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PAUL M. KENNEDY. *The Rise of the Anglo-German Antagonism, 1860-1914*. Boston: George Allen and Unwin. 1980. Pp. xiv, 604. \$60.00.

Central to the coming of the world wars, relations between England and Germany have attracted the attention of such historians as Friedrich Meinecke, Raymond Sontag, and most recently the British scholar Paul M. Kennedy. In order to transcend the limitations of "a political narrative," this massive study ventures into "comparative history" and into "the background social, economic, ideological and domestic-political factors which influence and in some cases dictate the course of foreign policy" (p. xi). Although his judicious diplomatic reconstruction holds few surprises, Kennedy sees already in the 1880s a "gap" between Britain and Germany" that was not deep enough to drive them into conflict but made "many members on each side . . . dislike . . . a closer connection" (p. 153). Intermittent attempts to form an alliance foundered for economic, geographic, ideological, social, and less so for attitudinal reasons: "The Anglo-German antagonism basically arose from the fact that in the half-century under scrutiny Germany grew out of its position as 'a cluster of insignificant States under insignificant princelings'; and from the further facts that this growth gradually threatened to infringe perceived 'British interests,' that these economic shifts increased the nervousness of British decision-makers already concerned about 'saving the Empire,' and that they were accompanied by ideas about a German mission which would be adopted by political forces grappling with severe domestic problems" (p. 466). Hence he assigns responsibility on two levels: "The immediate reply would be that it chiefly rests upon the German side." Since he admits that "the impact of German actions" tipped the "balance of forces" in London in favor of intervention *before* the violation of Belgian neutrality (pp. 457-58), Kennedy ultimately concludes: "Unless the Germans surrendered their desire—and their inherent capacity to alter the existing order in Europe and overseas; or unless the British were prepared voluntarily to accept a great change in that order, then their vital interests remained diametrically opposed" (p. 470).

This exhaustively researched work gives all indications of being definitive. Although partial to the Teutonic "tapeworm sentence," the style is gen-



erally smooth. The combination of chronological and topical approaches (even if diagramed unnecessarily) can only be applauded, since it brings into focus the full range of the ambivalent relationship. Many individual chapters, such as 3 and 15 on "Anglo-German Economic Ties" are exemplary in brevity, information, and clarity. Moreover, the author presents a mass of material in comprehensible fashion, draws well upon his primary sources in official and private collections of both countries, and dispatches ancient scholarly quarrels with authority. The range of topics—including the position and attitude of the parties, the press, pressure groups, and the public; the impact of religious and cultural connections as well as of monarchs and governments; and respective reflections on the weakening of liberalism and the role of the "official mind"—is truly impressive. Finally Kennedy's analysis of British policies, structures, and cross-pressures is imaginative and sympathetic.

Nevertheless, this accomplished work is also curiously disappointing. First, there is the problem of anticipated readership. The volume varies between unnecessary textbook generalities and surprising reticence in some essential points (such as the Boer War) that are assumed to have been treated elsewhere. Second, its breadth is largely mechanical, since the author is ultimately concerned with diplomatic decisions and concedes time and again that the cultural or other factors played little or no role. The potential paradigmatic synthesis of a new diplomatic history interrelating all dimensions therefore does not come about. Finally, the evaluative bias of the enterprise is reluctantly but unabashedly pro-British. The discussion of the German empire all too often deteriorates into critical clichés, informed by the recent sociostructural secondary literature that is no longer counterbalanced by older (or contemporary) favorable views. Moreover, underlying fatalism keeps the author from raising the crucial question of whether a more conciliatory British policy (the discredited appeasement of the 1930s) could have tamed the kaiser, limited German aspirations, and changed Berlin's abrasive style.

In the end Kennedy succeeds too well for his own good. By overdetermining the rise of the Anglo-German antagonism, he has created a new riddle: how could so many cultural, business, and even political leaders of such profoundly hostile countries strive so continually for a constructive relationship?

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BRUCE K. MURRAY. *The People's Budget, 1909–10: Lloyd George and Liberal Politics*. New York: Clarendon

Press of Oxford University Press. 1980. Pp. ix, 352. \$49.95.

The political furor over the "people's budget" was not just a matter of "the people" versus "the peers." The budget was very much a challenge to tariff reform, an assertion that free trade finance could work and, unlike tariff reform, would shift the balance of taxation away from indirect taxes toward direct taxation.

Although the budget represented a turn to the left by the Liberal party and can be deemed a major example of the "new liberalism" of the period, it was nevertheless a fiscal package that took care to soothe the larger part of the middle class. Sir Edward Grey, campaigning for the January 1910 general election, was quick to point out that the professional man with an annual earned income of under £2,000 would pay less income tax under the 1909 budget than he had under the last Unionist government. *The Labour Leader* complained that the budget operated mainly "in the interest of the smaller middle class" but nevertheless welcomed it as "the beginning of a system of taxing the wealth of the rich rather than the poverty of the poor."

The "people's budget" did break new ground in tax raising, just as Harcourt's celebrated 1894 death duties budget had done—so much so that the permanent head of the Treasury in 1909, struggling to estimate the expected revenue, observed that "the whole thing is a leap in the dark, and we have absolutely no experience to guide us." One of the many merits of Bruce K. Murray's book is that he does not belittle the innovative nature of the budget.

Lloyd George, who always had a knack for presenting relatively modest proposals in a fiery way, had no difficulty whatsoever in raising the political temperature over his budget. Later Philip Snowden was to remark that Lloyd George attacked the landed interests "with a violence which would have done credit to a communist agitator." At the time of his famous Limehouse and Newcastle speeches in October 1909, Lloyd George may well have been out to provoke the House of Lords to reject the budget; but earlier, as Murray argues, there is nothing to suggest that the budget was framed with the intention of bringing about a final crisis between the House of Lords and the Liberal government.

Murray's book not only examines the budget in detail but also explores the politics of 1908–10, providing a perceptive discussion of the aims and activities of the Liberal ministers, the Unionist leaders, the Labour party, and the Irish MPs. The main criticism of the book must be that it could have benefited from some pruning, especially in the introductory chapters. However, this criticism should not overshadow the fact that *The People's Budget, 1909–10* is a major contribution to the study of Ed-

wardian England and deserves to be the standard work on its subject for a very long time. The book can also be recommended to the members of Margaret Thatcher's cabinet, virtually all of whom only saw the 1981 budget on budget day. In contrast, Asquith's cabinet scrutinized Lloyd George's 1909 budget almost clause by clause and line by line, although like the present cabinet the majority of them did not like what was proposed.

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GAIL BRAYBON. *Women Workers in the First World War: The British Experience*. Totowa, N.J.: Barnes and Noble or Croom Helm, London. 1981. Pp. 244. \$24.50.

Was World War I a watershed in the history of British women? Arthur Marwick and others have claimed that it was and have suggested that this was an important example of war stimulating social change. Gail Braybon's study is significant in part because it demonstrates that Marwick has overstated his case concerning the impact of World War I on British women. It also casts doubt on his broader thesis about the relationship between war and social change.

In *Women Workers and World War I* Braybon restricts her analysis to British working-class women workers. Within this area she provides new evidence concerning two central issues: (1) the extent to which the war changed public attitudes toward women working and (2) the way in which these attitudes shaped the self-image of women workers.

The increase in the number of women workers during World War I and the employment of women in jobs previously reserved for men have caused some to claim that job opportunities for women were profoundly altered by the war. But Braybon disagrees. The trade unions allowed women to be employed on "men's jobs" only under the condition that following the conflict the prewar division of jobs into men's work and women's work be restored. Public hostility toward women working gave way to approval during the war but revived in 1919 when many women sought to retain their positions. By 1921 the percentage of women who were gainfully employed was less than it had been in 1911.

Braybon correctly notes that "equal pay for equal work" became an important public issue during the war but argues that the increased support for this reform did not imply acceptance of the justice of the women's claim. Equal pay was supported by many antifeminists: some advocated it because it would protect men's wage rates, while others considered it a means of keeping women out of indus-

try after the war. Perceptive feminists warned against "equal pay for equal work" on the ground that it would benefit only the small minority of women who continued to be employed on men's work after the war; for the main body of working-class women workers it would bring no improvement since they were engaged in "women's work."

Their wartime experiences altered the attitudes and aspirations of working-class women workers, and, as a result, many wished to retain their wartime jobs after the conflict. But public opinion contributed to a revival of prewar sex role stereotypes. Even women trade union and Labour party leaders accepted the assumption that "motherhood was women's most important task" and encouraged women industrial workers to leave their wartime jobs (p. 166). Women who continued to work in the 1920s, even on women's jobs, faced greater public disapproval than prewar women workers did.

Apart from the William Beveridge papers, Braybon's study is based on published sources and thus has little new to add to previous accounts of the process of policy making within the government. There are relatively few factual errors: the Restoration of Pre-War Practices Act is consistently mislabeled (pp. 174, 184, 242), and Eleanor Rathbone is erroneously referred to as a member of the Labour party (p. 98). But these are minor cavils about a fine monograph that should be consulted by anyone concerned with the effect of World War I on British society.

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TOM STANNAGE. *Baldwin Thwarts the Opposition: The British General Election of 1935*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1980. Pp. 320. \$50.00.

"A piece of sharp practice" (p. 134), editorialized the *Manchester Guardian*, correctly anticipating that the National Government would call a general election in the autumn of 1935, at a time when the country stood united behind the League of Nations and collective security. Yet opponents of the coalition government headed (since the preceding June) by Stanley Baldwin doubted the government's commitment to support these principles, fearing that the regime was bending its knee to the sentiments voiced in the "Peace Ballot" for electoral purposes only. When a handsome coalition victory was followed within the month by the abandonment of sanctions and the discrediting of the League, the scepticism of October was confirmed. Since that time, historians have generally concluded that the politically astute Baldwin had in-

deed engaged in "a piece of sharp practice" in order to assure the return of the National Government to power. Indeed, the House of Commons elected in November 1935 sat as "a long Parliament" beyond all expectation through all but the final month of World War II. It is a considerable virtue of Tom Stannage's painstaking analysis of this election that these views are confirmed: Baldwin skillfully manipulated the political levers available to the government in order to secure electoral victory. Save for his handling of the abdication crisis, the election of 1935 stands as the crowning triumph for the master of interwar politics, whose reputation has never recovered from such success.

Stannage's book is cast in the mold of "electoral geography" pioneered in 1947 by R. B. McCallum and Alison Readman in *The British General Election of 1945*; he has adapted a methodology to an election nearly two generations removed, even to the extent of calculating the "issue" content of candidates' addresses. Most dubious among the methodological schemes is Stannage's attempt to depict "the public mind" (p. 153) in the absence of public opinion polls. In the more readily quantifiable aspects of the election, Stannage acquits himself in a persuasive fashion. The most useful conclusions derived from his efforts are that the Labour party failed to regain about one-third of the ground lost to the Conservatives in the disastrous 1931 election and that somewhere between one-quarter and one-third of the vote lost by the Liberals since 1929 went in 1935 to the Conservatives rather than to Labour. Until these trends were reversed, Labour was nowhere near to securing the electoral breakthrough that came a decade later.

Yet in common with accounts of the elections since 1945, *Baldwin Thwarts the Opposition* is much more than a close study of the election; instead Stannage's purpose is to assess the course of electoral politics since the 1931 election. In view of the unsurprising description of the 1935 proceedings, the most interesting facet of the book is the description of those influences that shaped the fortunes of the three coalition and two opposition parties as they prepared to contest that election. The election proper is cross-sectioned: its timing, development, and issues are carefully scrutinized, as are the roles of the mass media, the candidates, and the outcome. The campaign itself never caught fire—despite some scattered Tory hopes to ignite the politics of fear, "the only way to make a great many people go to the poll" (p. 121), no "scare" was needed, unlike 1931 and 1924—and the lack of excitement that Stannage finds in "so orderly an affair" (p. 11) takes its toll as well upon his book. The shortcoming is deeper because Stannage's "electoral politics" nearly abandons politics altogether: the flesh and blood of political activity—Labour's leadership

crisis, or the relations of the parties to the "Peace Ballot" (incidentally, since the vote on EEC membership, no longer "the only national referendum" [p. 155] in British history)—is held subservient to a structural analysis.

Stannage's indefatigable labors command respect; in addition, his description of the ancillary features of the campaign, such as the novel impact of sound newsreels or the gathering force of the BBC's commitment to consensus and thus to the National Government, are informative. Because of this book, we know all that we need about the setting of the 1935 election; what we lack are actors upon the stage who do more than read the lines of their campaign addresses and party manifestoes.

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JOHN HORSFIELD. *The Art of Leadership in War: The Royal Navy from the Age of Nelson to the End of World War II*. (Contributions in Military History, number 21.) Westport, Conn.: Greenwood Press. 1980. Pp. xiv, 240. \$25.00.

This is a highly readable book based on wide reading in both the primary and secondary sources and written with great zest and obvious concern for its subject. It is in essence an inquiry into the nature of the leadership of the Royal Navy, which for centuries kept Britain immune from invasion, made it the dominant imperial and trading country in the world, and played an essential part in the defeat of its enemies in two world wars. By analyzing the achievements of selected admirals from Nelson and his contemporaries to Andrew Cunningham in World War II, John Horsfield seeks to establish the qualities and skills that shaped a long tradition of the pursuit of victory by aggressive strategy and tactics unique in the history of warfare.

The study of the individual admirals is preceded by a brief discussion of the problems involved in identifying the essential components of successful command at sea and an even briefer survey of previous writing on the subject. If this is the least incisive part of the book, it is partially due to the intractability of the subject and the failure of either sociologists or the more traditionalist biographers and moralists to produce totally acceptable answers. It seems probable that the question of whether leadership is inherent or learned, which has puzzled many previous writers, falls into the same insoluble category as that of whether intelligence is determined by inheritance or environment.

Horsfield's historical analysis is based on four themes: the admirals' relations with their superiors, colleagues, and subordinates; their competence as

strategists and tacticians; their administrative abilities; and their personal and social backgrounds. His conclusions lead him to identify three aspects of leadership: those that are variable and apply to particular historical circumstances, those in harmony with various national traditions, and, very tentatively, those that are universally applicable. There is nothing startling or original here, and there is no question of the book opening up new waters to the specialist. Moreover, although general readers will find much to interest and inform them, they must be warned against some omissions and questionable judgments.

Too little attention is paid to the impact of technological change on modes of leadership. Horsfield's omission of the sternest test of Cunningham's leadership, the acceptance of heavy losses in the evacuation of Crete, based as it was on a glowing sense of the navy's tradition never to leave the army in the lurch, is even more surprising. The unfavorable verdict on Jellicoe is based too much on the views of prejudiced critics and pays too little attention to all the factors that caused Jutland to be indecisive, especially the thinking of the German commander, Scheer. There is no denying the claim of Nelson to be the epitome of British naval leadership, but it is doubtful that the search for a twentieth-century successor bulked as large in the concerns of the Royal Navy as Horsfield maintains.

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JOHN RAMSDEN. *The Making of Conservative Party Policy: The Conservative Research Department Since 1929.* (A History of the Conservative Party.) New York: Longman. 1980. Pp. 324. \$50.00.

The modern Conservative party, unlike its Labour and Liberal rivals, has attracted relatively little historical attention. This is not surprising. When compared to its competitors the party appears to offer little of the drama of a party in decline or a party on the march toward office. By virtue of having been there for so long, the Conservative party seems intrinsically less interesting. Recently, however, this state of affairs has begun to change. In two distinguished books published within two years of one another, John Ramsden has firmly established his credentials as the leading authority on the history of the modern Conservative party. The political history of the party was ably presented in his volume, *The Age of Balfour and Baldwin, 1902-1940*. That book provides part of the background against which he now undertakes an examination of how and by whom Conservative policy is actually made.

At the center of the policy-making apparatus for

over fifty years has been the Conservative Research Department. That the party should think systematically about policy only became something of an issue with the advent of Chamberlain's tariff reform campaign, the emergence of the Labour party, and the reformist activities of the prewar Liberal governments. It was not, however, until 1929, in the aftermath of the Conservative party's disastrous loss to Labour, that Baldwin most reluctantly agreed to the establishment of a research department. Neville Chamberlain had been the most persistent advocate of such an organization, and to this day the department faithfully reflects many of his concerns. Its responsibilities are threefold: the provision of data and briefs for individual MPs, party leaders, and party committees; research on intermediate and long-term policy questions; and assisting in the preparation, vetting, and publication of a good deal of the party's literature.

For most of its existence the research department has successfully maintained its position as an instrument of the party leader rather than of the party organization. Inevitably, therefore, the department's fortunes have been closely tied to the political fortunes of the leader and his or her view of its place in the policy-making process. Churchill, for example, largely ignored the department, and this allowed its chairman, R. A. Butler, considerable freedom to lay the fundamental and more theoretical foundations for the recovery of party fortunes in 1950 and 1951. From 1964 to 1970 Edward Heath presided over the most careful and thorough policy preparations for the Conservative party's return to power. The party, however, found itself over prepared for office; rigidly committed to a policy timetable, it lacked the flexibility needed to deal with the unexpected domestic and external challenges of 1973-74. Under Margaret Thatcher the pendulum has swung strongly in the other direction. The research department is no longer the sole instrument of party research (its first competition emerged in 1966), and ideology rather than policy has come to the fore again.

The primacy of principles or ideology as opposed to policy making, the political fortunes of party leaders, and whether or not the party has been in power have shaped the fortunes of the research department since 1929. But what is it that has made the Conservatives the dominant political force in modern Britain? To answer this question historians need to explain, as Henry Brooke wrote in 1944, "what the Conservative philosophy really is" (p. 101). Ramsden has thoroughly prepared the ground for the next stage of the inquiry.

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DEREK BIRRELL and ALAN MURIE. *Policy and Government in Northern Ireland: Lessons of Devolution*. Totowa, N.J.: Barnes and Noble or Gill and Macmillan, Dublin. 1980. Pp. 353. \$32.50.

This book touches on many of the perplexing questions that have made Northern Ireland both a political battleground and a fruitful field of academic debate about devolution. Examining the relations between Stormont and Westminster, Derek Birrell and Alan Murie suggest that in practice the former's status was closer to the federal model than the devolution model in that the two governments were almost "co-ordinate with each other, each with its own sphere of influence" (p. 29). Northern Ireland did enjoy considerable internal autonomy, and the main purpose of *Policy and Government in Northern Ireland* is to show how the regional government and its component parts operated in recent years. There is also a chapter on direct rule and a postscript on possible future forms of government.

In such an ambitious analysis, there are bound to be omissions and imbalance. Thus the space devoted to the activities of religious organizations and the Confederation of British Industry serves to emphasize the authors' failure to show how rural pressure groups operated and the way in which rural district councils acted as the mouthpieces of the farming community. It is also difficult for the historian to appraise the often chronologically promiscuous work of social science. The authors, themselves social scientists, do have a sense of history and show how the nature of political processes and the rate of development of institutions varied over time, but the historical perspective is patchy. Whereas an informed discussion of education goes back to the 1920s, the more perfunctory remarks on religious discrimination in the civil service fail to recreate the atmosphere and tensions of the crucial formative years. Moreover, the work is rendered less authoritative by the fact that, owing to restrictions on access, most of it could not be based upon the working papers of the government of Northern Ireland, while it is, perhaps, too dismissive of the conclusions drawn by other studies more firmly based on the archives.

Nevertheless, *Policy and Government in Northern Ireland* is indispensable as a work of reference and a contribution to the debate on devolution in the United Kingdom. There are interesting data and comments on the Northern Ireland cabinet, parliament, pressure groups, civil service, local government, and, most originally, social policy, and the overall conclusion, "that the adoption of a devolved system itself solves nothing" (p. 301), is a sound one. "It is how devolution operates and how other institutions develop alongside it that determines the nature of the remaining problem. . . . If it is too much

to expect devolution to solve the regional problem in terms of economic and social provision, perhaps more attention should be given to the style of government, the quality of administration, the rights of citizens and participation in decisions" (pp. 301-02).

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PHILIP BENEDICT. *Rouen during the Wars of Religion*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1981. Pp. xx, 297. \$39.50.

This study is a brave but flawed attempt to apply the techniques of quantitative social history to Rouen during the religious wars. Its focus is on the beginning of the wars, the massacre of St. Bartholomew's Day, and the period of League supremacy. Such success as it enjoys is based on a masterful comprehension of the Rouen archives and an ability to choose and arrange material in statistical order and to relate it to the most up-to-date scholarship. Its failure lies in the way in which it conceptualizes the religious conflicts of the period.

The rivalry of Protestantism and Catholicism is presented legitimately enough as that between two *mentalités*. On the other hand, these *mentalités* are depicted as if held by two groups of discrete individuals—mainly laymen—whose characteristics can be statistically analyzed. There is no apparent understanding of the significance of Catholics as part of an enormously powerful, wealthy, and intrusive institution dominated by an aristocratic upper clergy. Nor does this work offer any sense of the effect of the enormous weight of such an institution on shaping the *mentalité* of those who chose to remain loyal to it or those who chose to revolt from it.

Philip Benedict does supply us with an excellent portrait of Rouen's economy and social structure based on his extraordinary grasp of the sources. But he fails despite his best efforts to overthrow the Hauser thesis, which sees the Reformation in the first place as the economic and social protest of the artisans. In order to discount Hauser's view, Benedict pointedly ignores the craftsmen in the suburban and rural cloth industries dependent on Rouen. In the case of the faubourg St. Gervais he persistently refers to it as "semi-agricultural," although he could have referred to it as semi-industrial, since it was peopled by rural weavers who show up in the parish registers of 1572 as Protestants.

Benedict dogmatically denies any relationship between economic difficulties and religious violence (p. 58) and then fifty pages later grudgingly concedes it (p. 111). But his real failure is an inability to conceptualize the relationship between religious



and economic conflict properly. The poles of such a conflict do not consist, as he believes, of rival blocks of townsmen so much as of a church controlled by an aristocratic or aristocratized upper clergy, whose real income was increasing until at least mid-century, and an artisan population, especially members of the secondary and export sectors, whose real income was declining in the face of the sixteenth-century inflation.

If Benedict is unable to deal with the genesis of the Huguenots very successfully, he does a masterful job in accounting for their decline in the wake of St. Bartholemew's Day. His account of the League, which in part overturns that of Henri Drouot, is most persuasive as well.

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JEAN-LOUIS THIREAU. *Charles du Moulin, 1500-1566: Étude sur les sources, la méthode, les idées politiques et économiques d'un juriste de la Renaissance.* (Travaux d'Humanisme et Renaissance, number 176.) Geneva: Librairie Droz. 1980. Pp. 459.

Legal historians have always realized the great importance of Charles du Moulin, but historians at large have been hampered from doing so for two reasons: most of Du Moulin's voluminous corpus of writings was composed in Latin, and his thoughts on critical issues were often scattered in different treatises. Jean-Louis Thireau ameliorates these conditions by his judicious (though not skimpy) selection of pithy Latin passages to quote in footnotes, and by adopting a thematic scheme that allows direct access to Du Moulin's ideas from many avenues of approach. After a straightforward biographical summary, Thireau deals at roughly equal length with law and legal methodology (divine and natural, Roman and customary, Bartolian-humanistic), political thought (royal sovereignty, church and state), and economics (usury, *rentes*, monetary theory).

Reformation historians will find Du Moulin's life exemplary of the religious tensions of his time: he was drawn into the Reformation by the attraction of Luther's theology, left it due to the repugnant nature of Calvinist ecclesiology, and died reconciled with the Catholic faith. In like fashion the eddies and currents of Renaissance jurisprudence all moved in his legal works. The chief one, the commentary on the custom of Paris, validated the autonomy of local law but at the same time suggested the superiority of Paris's *coutume princesse*. At times Du Moulin even pursued the elusive ideal of a common law for all of France. And Roman law, which he asserted repeatedly should remain silent until French law showed itself deficient in some respect,

he used instinctively, without apologizing, because of its inherent order and the existence of masterful commentaries on it.

In the realm of political thought, one small but noteworthy revelation emerges in the few pages where Thireau assembles Du Moulin's scattered reflections on the "king's two bodies." (As the book of that title by Ernst—not Hermann—Kantorowicz shows, French jurists rarely dealt explicitly with this notion as did their English counterparts.) According to Du Moulin, the head of the "body politic" was not the prince but the principate; that is, not the king alone but the king along with "integrated members," those associated with him in rulership. More constitutionalism seems to be imbedded in Du Moulin's royalism than historians of political thought have realized. Lastly, in the sphere of economics, Du Moulin helped cleanse the theory of *rentes* from the stain of usury and opened the way for their use as an instrument of capital. The nature of *rentes* is still today so regularly misunderstood that Thireau's exposition is fully warranted—and it proves to be greatly rewarding.

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BAILEY STONE. *The Parlement of Paris, 1774-1789.* Chapel Hill: University of North Carolina Press. 1981. Pp. x, 227. \$19.00.

The position of the Parlement of Paris in the last years of the *ancien régime* has long been perplexing, for the chief magistrates of France were apparently implacable opponents of both arbitrary government and progressive reform. As Bailey Stone shows in his historiographical introduction, this conundrum is often solved by condemning them as the hypocritical leaders of an "aristocratic reaction" that supposedly precipitated the French Revolution.

Stone, whose extensive research was encouraged by the late Jean Egret, seeks to clarify the problem by systematically analyzing the attitudes of the magistrates, between 1774 and 1786, toward the administration of justice and toward the monarchy, the aristocracy, and the commons. The crisis of 1787-88 is then reconsidered in the light of this analysis. In this approach some important issues, like those involved in the question of the *corvée*, inevitably recur in different contexts. Less necessarily, rigorous concentration upon the specified inquiry excludes some desirable background. The magistrates' earlier relationships with the monarchy, and with other parlements throughout the period, are probably more important to an appreciation of the subject than Stone allows. Thus he says strangely little about the *parlementaires'* propaganda, which is usually prominent in indictments of them. It also

seems a pity that the searching analysis of attitudes should lead to such brief treatment of the concluding crisis. The work is, in short, a thesis, in the publication of which no concessions have been made to those not already well informed.

Although Stone still favors the current jargon in which some factors "boost" or "highlight" others, thus "sparking" crises and revealing points to be "underscored," he generally writes with clarity and authority. His magistrates appear as real people, whose various opinions he holds in admirable balance. Amid many complexities he discerns important continuities of professional attitude, including much genuine concern for the welfare of working people and a complete inability to comprehend the monarchy's imperative need for a greater regular revenue. Central to his analysis, however, is the idea of ambivalence: despite their intransigent opposition to "ministerial despotism," despite even their eagerness to increase their own corporate privileges, the magistrates are seen as primarily interested in maintaining a "natural order" in which each component part of a pluralist society, including both monarchy and aristocracy, had its proper place. Stone consequently questions the legitimacy of identifying the robe with the sword, for he finds that the aristocracy of magistrates opposed the pretensions of the nobility in general. He further finds it natural and indeed inevitable that in September 1788 the Parlement determined that the Estates General should be constituted as it was in 1614.

Ultimately Stone differs even from Egret, for he is unable to accept the view that some magistrates deliberately betrayed the people for the sake of an aristocratic class. His book thus makes a considerable contribution to our understanding of an important subject.

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TOBY GELFAND. *Professionalizing Modern Medicine: Paris Surgeons and Medical Science and Institutions in the Eighteenth Century*. (Contributions in Medical History, number 6.) Westport, Conn.: Greenwood Press. 1980. Pp. xviii, 271. \$29.95.

In 1794, the Thermidorean Convention established a new *école de santé* in the buildings of the former College of Surgery, constructed only two decades earlier. The move symbolizes the main point of Toby Gelfand's well-argued study. The chairs of clinical surgery and clinical medicine at the new school were initially held by the surgeon Pierre-Joseph Desault and one of his former students, the doctor Jean-Nicolas Corvisart, two men who exemplify the professional amalgamation that had oc-

curred. Gelfand argues that Desault, whose importance previous scholars have recognized, was no lone genius, but the product of an institutional transformation of surgical practice over the century. Before making the ancient Paris Hôtel-Dieu into a model of clinical instruction in the last years of the old regime, Desault was molded by his own experience at the *école pratique* of the College of Surgery and at its hospice and by his service in the clinic of the Paris hospital of La Charité. Other developments in medicine and hospital care also shaped his main ideas: separate operating rooms where students observed surgery, student responsibility for patient care and the keeping of case histories, and the correlation of autopsies with clinical observations.

How did surgeons manage to displace the antique verities so jealously guarded by the Paris Faculty of Medicine and impose their own medical epistemology? Working from a well-articulated concept of professionalization, Gelfand finds the answer primarily in social history and in the role of bureaucracy. The uniting of the learned guild of surgeons "of the long robe" with the rude barber-surgeons in 1655 appeared to accentuate the ascendancy of medicine over surgery. In fact, it consolidated the most numerous group of medical practitioners—the multi-tiered guild of surgeons—and gave them sustained royal patronage. The "monarchical" authority wielded by the king's premier surgeon—La Peyronie being only the most outstanding of these—helped the surgeons construct their institutional base.

The advantages they gained produced a wave of "contestations" with the medical faculty in the 1740s and a crucial ideological shift. François Quesnay, the later sage of physiocracy, defended surgeons in the 1730s as empirically trained craftsmen and then shifted in the 1740s to arguing for the unity of theoretical and empirical knowledge in healing. Gelfand identifies in the 1740s a new dichotomy between strictly routine mechanical trades and the "worthy" manual arts that required theoretical understanding. By the end of the century, an elite of learned surgeons and doctors governed a "unified, two-tiered profession," while a smaller number of "health officers" performed, for smaller rewards, medical services requiring a lower degree of skill.

The surgeons' status was advanced by royal exclusion of "barber-wigmakers" from the practice of surgery, a move that tended also to dissociate barbers from surgeons. At the same time, scientific prestige accrued to the feats of better-trained surgeons. The disparity between patients' needs and the ratio of surgeons to doctors led surgeons to extend a *de facto* practice of internal medicine, while a "social gradient" drew ambitious sons of surgeons

into the circle of doctors. By taking medical degrees, learned surgeons advanced their social status and gained the legal right to charge fees for their advice. Further studies will no doubt measure all these trends in greater detail.

Gelfand's work is a useful contribution to the history of medicine, to the history of professions generally, and to the study of bureaucracy and social change in the eighteenth century. He has enriched a field already distinguished by the work of scholars as diverse as Michel Foucault and Oswei Temkin.

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PATRICK J. HARRIGAN. *Mobility, Elites, and Education in French Society of the Second Empire*. Waterloo, Canada: Wilfrid Laurier University Press. 1980. Pp. xv, 203. \$8.50.

Numerous scholars have asserted that nineteenth-century French secondary education was elitist and bourgeois and that it restricted social change. Dissatisfied with the thin documentation and vagueness of such claims, Patrick J. Harrigan has investigated the interplay among secondary schools, social mobility, and elitism. Much of his analysis is based on a remarkable 1864 survey that provides data on the social origins, occupational hopes, and post-graduate careers of about 28,000 students and recent alumni of France's male public *lycées* and *colleges communaux*. Subjecting this data to a variety of sophisticated statistical techniques, Harrigan arrives at fresh and important insights.

He demonstrates that, contrary to received opinion, secondary schools were not populated exclusively by the sons of the rich and powerful. Instead, about 40 percent of the students were from peasant and petit bourgeois backgrounds. Moreover, the graduates in the survey experienced a surprising amount of upward and even downward mobility. Secondary schools thus actually contributed to substantial social fluidity, more than either their planners had envisioned or historians have recognized. Harrigan concludes that although the system was concerned with forming an elite, it did not simply preserve existing social patterns or even assure access to power. Instead, "passage through secondary schools had become a rite through which old elites demonstrated their vitality while renewing themselves by the infusion of the most talented and ambitious sons of the middle classes" (p. 152).

The book is peppered with a number of interesting subsidiary arguments. For example, suggestive references are made to the impact of secondary education on geographic movement. Unfortunately this analysis remains incomplete, perhaps because

the data was less pertinent to this topic. Thus, it is unclear whether secondary schools did more to infuse the countryside with talented young men or to drain them from it.

Harrigan is able to make better use of his evidence to evaluate the effect of students' family backgrounds on their occupational hopes, schools attended, and program choices. He also assesses the relation of all four of these factors to students' graduate careers. Among other findings, he establishes that, save for the smallest ones, communal colleges were not as inferior to *lycées* in terms of the social origins of students or their graduate achievements as is often assumed. Instead, the major dividing line was programmatic. Students enrolled in the classical curriculum in either type of school were from higher status groups and had much greater access to high status positions than students who pursued the nonclassical, special program.

To extend the import of the study, Harrigan compares his findings to sociological models of the relationships among mobility, modernity, and education. He concludes that Second Empire France offers little support for current theories. For one example, job mobility among graduates did not seem to be a concomitant of industrialization since movement from traditional to modern occupations was more pronounced in poor and agricultural areas than in wealthy and industrial regions.

In sum, *Mobility, Elites, and Education in French Society of the Second Empire* is an important book. Its data base, analytical techniques, and thoughtful interpretations make it useful not only to historians but also to social scientists interested in the relationships between schools, social change, and elite formation.

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ANNE LOMBARD-JOURDAN. *La Courneuve: Histoire d'une localité de la région parisienne des origines à 1900*. (Centre Régional de Publication de Paris, Institut de Recherche et d'Histoire des Textes.) Paris: Éditions du Centre National de la Recherche Scientifique. 1980. Pp. 246. 80 fr.

This magnificently illustrated book attempts to chronicle the history of La Courneuve, a town near Paris, from its foundation until 1900, and it attempts to do this in fewer than two hundred pages of text. Scholars influenced by recent social science methodology will find this an outmoded approach. Anne Lombard-Jourdan's book is a good example of the sort of village history that was written before the rise of microanalysis, be it the approach of the *Annales* school or of historical demography.

Trained at the École Nationale des Chartes, Lombard-Jourdan is a medievalist, and the chapters on the pre-Christian era and the early Middle Ages are the best in the book. Especially interesting is the use of archaeological evidence to establish the probable existence at La Courneuve of a gathering place for worship of the sun. The belief in magical springs is also documented.

The rest of the book is much less inspired. We learn of La Courneuve's domination by the Abbey of Saint-Denis, the establishment of different settlements during the Middle Ages, and the life of the nobles at the Château de Sainte-Foi. Attention is given to the increasing complexity of political life in the nineteenth century, with perhaps too much focus on a rather predictable quarrel between the priest and the schoolteacher. The growth and differentiation of La Courneuve in the nineteenth century are described, and the importance of a good transportation and communications network for the integration of La Courneuve into the Parisian area is stressed.

The main problem with Lombard-Jourdan's approach is that it never rises above the purely descriptive. There is virtually no philosophy of history, no statement, explicit or implicit in the text, about why village history should be written. Description and detail seem to be Lombard-Jourdan's entire goal. They are important, to be sure, but the best historical studies of French villages written in recent years, such as Patrice Higonnet's *Pont-de-Montvert* and Thomas F. Sheppard's *Lourmarin*, have tended to be analytical as well as descriptive and to advance theories about the operation of French society.

The commune of La Courneuve actively participated in the publication of this book, and it is probably in its schools that the book will be most useful. The wealth of maps and illustrations, some especially rare and beautifully reproduced, will make this book meaningful to anyone who already knows La Courneuve and its land. For academic historians, whose interests are necessarily more abstract, this book will be less satisfying.

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JAMES SMITH ALLEN. *Popular French Romanticism: Authors, Readers, and Books in the Nineteenth Century*. Syracuse: Syracuse University Press. 1981. Pp. xiii, 290. \$20.00.

James Smith Allen's *Popular French Romanticism* presents an interesting argument concerning the relationship between high and low culture during the first half of the nineteenth century and particularly

during 1820–40, the period of romanticism's greatest popularity. Allen maintains that during this period the romanticism of major writers filtered into popular literature and that through this vehicle the values and sensibilities—the paradigm—of romanticism reached a popular audience. For the first time since the sixteenth century the rift between high and low culture was closed, not completely, but substantially. The newly gained unity did not outlive the period of romanticism, however, and paradoxically that unity broke down because of the connection between the two literatures. When popular writers took over the apparatus of romanticism, they carried to excess the motifs and devices of serious authors; those excesses helped feed the reaction against romantic extravagance after 1840 and thereby contributed to the naturalism of the next generation. As serious writers struck off in a new direction, popular literature retained and even carried further the exoticism, suspense, and emotional appeal of romanticism. After 1840 there was again a separation between high and low culture. Only the latter, fed by cheap newspapers and their serialized fiction, became a mass culture that was part of modern mass society.

Allen's argument is interesting and largely persuasive. It rests upon a study of randomly selected popular romantic works, drawn from some 100,000 titles in the *Bibliographie de la France* for the period 1820–1840. Allen chose a total of 120 titles from 1820, 1827, 1834, and 1841, and for each of these years examined works of poetry, drama, fiction, and history. From his study of the selected works and the book industry that produced them, he reaches conclusions that are always judicious but that do not quite add up to the sum of their parts. One learns of "The profound impact romanticism had on . . . plebeian literature" (p. 43); that popular romanticism, like romanticism itself, was ideologically ambivalent, at once seeking stability and order in a hierarchical past and openly embracing the "new world of revolution and empire" (pp. 71–72); that the 1830 revolution politicized literature, both serious and popular, tilting the scales in favor of social themes and commitment to progress rather than Gothic escapism (p. 200); and that major romantic writers were sometimes contemptuous of the bourgeoisie, while popular romantic writers flattered the middle-class readers who made up their essential audience (pp. 215–16).

As useful as these points are in explaining the relationship between literature and society in France during the first half of the nineteenth century, they are scattered throughout the book rather than presented systematically. Another problem is that some of the chapters are organized loosely and tend to be diffuse. Fragmented as the book sometimes is, however, it will repay the careful study



that it requires if its ideas and implications are to be understood.

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MICHAEL B. MILLER. *The Bon Marché: Bourgeois Culture and the Department Store, 1869-1920*. Princeton: Princeton University Press. 1981. Pp. xii, 266. \$13.50.

In *The Bon Marché* Michael B. Miller has traced the early history of what was probably the world's first department store. Beginning with its opening in 1869, Miller assesses the monumental success achieved by the founders, Aristide and Marguerite Boucicaut. By 1900 the Bon Marché was one of Paris's tourist attractions, the center of the evolving consumer culture, and a significant factor through its advertisements and catalogues in making that culture of consumption a national phenomenon (p. 166).

Miller's history of the store begins with a chapter on the simultaneous retailing revolutions in England, France, and the United States as dry goods merchants diversified beyond the standard lines of yard goods and clothing. This retailing revolution of the 1840s and 1850s involved fixed pricing and lower profit margins and was facilitated by a heavier volume of sales and high stock turnover. Self-promotion became a way of life at the big stores, for they depended on a wider clientele than small shops. Store catalogues, spectacles, and advertising were innovations of the stores that had a tremendous impact on nineteenth-century culture. "In one respect, the Bon Marché came to serve essentially the same role as the Republican school system," writes Miller. "It became a bourgeois instrument of social homogenization, a means for disseminating the values and life style of the Parisian upper middle class to French middle-class society as a whole" (p. 183).

As Miller argues, the Bon Marché was widely emulated and was a model of its type. Thus the history of the Bon Marché provides a fascinating, if unique, case study of managerial paternalism in the age of transition. Yet, although Miller claims that this is no simple business history (p. 6), his book at times displays the same problems as earlier store "biographies" such as Asa Briggs's paean to Liberty's of London, *Friends of the People* (1956).

Significantly, Miller begins his book in 1869—a year that was marked by a watershed strike of department store clerks—but chooses to ignore the strike and concentrate on the inauguration of the new building of the Bon Marché. While brilliant in his insight into middle-class values, Miller seems to

share the management's view that employees were mere pawns of store policy. As Miller himself points out, the department stores were as much a bourgeois world because of the white-collar clerks as for the life style of their managers (p. 109). Both its customers and its clerks made the Parisian department store a "monument to the bourgeois culture that built it, sustained it, marveled at it, found its image in it" (p. 3).

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WILLIAM R. BEER. *The Unexpected Rebellion: Ethnic Activism in Contemporary France*. New York: New York University Press; distributed by Columbia University Press, New York. 1980. Pp. xxxii, 150. \$18.50.

William R. Beer's study examines the contemporary resurgence of ethnic activism in France on the part of Bretons, Corsicans, Alsations, Occitans, Basques, Catalans, and Flemings. Historical and theoretical discussions are followed by an analysis of the social characteristics of leaders in order to ascertain the origins, development, and prospects of such ethnic activism. In each case the findings of this slender volume (less than three-fifths of its 150 pages are devoted to text) are disappointing.

The historical chapter is poorly organized, superficial, and replete with errors. To attribute the revival of ethnic activism in the 1960s to such general causes as modernization and government policy in Paris or to political factors like decolonization, the example of other ethnic movements around the world, and the upheaval of May 1968 is far too vague to be very useful. It also means that Beer overstates external factors in the generation of ethnic discontent at the expense of an analysis of internal conditions within ethnically distinct regions. Similarly, he explains the ideological complexion of ethnic activism at any given time as no more than the reactive adoption by its leaders of the opposite of whatever ideological pattern prevails in Paris. In any event, to read that Napoleon abolished the historic provinces of France and substituted departments for them (p. 2) or that the daughter of Duke François of Brittany married French King Louis IX in 1532, when in fact both of them were long since dead (p. 5), hardly inspires confidence in the author's understanding of French history.

In the theoretical section of this book (chaps. 2 and 3) Beer seeks to test hypotheses drawn from theories of internal colonialism, rising expectations, and relative deprivation. He does this by correlating ethnic activism as expressed by votes for so-called federalist candidates in the French presidential election of 1974 and by nonelectoral forms of ethnic dissent like bombings and demonstrations with con-



ventional statistical measures of economic development in the departments of ethnically distinct regions of France. All of this is most unconvincing. The vote for such federalist candidates, as Beer himself admits, was miniscule, while the other dependent variable of nonelectoral dissent is manipulated not in terms of the statistical incidence of such phenomena as bombings and demonstrations but rather as a subjectively constructed rank ordering of the various regions. Such methodological mixing of apples and oranges simply cannot generate credible results.

The chapter on the social characteristics of ethnic leaders is likewise flawed. Such individuals in Corsica—where ethnic activism in recent years has been more militant and violent than in any other French region—were not studied because “it would not have been practical to visit Corsica” (p. 137 n. 1). It is difficult to see why this would have mattered since the survey of ethnic leaders elsewhere was conducted via a mailed questionnaire. The use of such an instrument drafted solely in the French language is in any case questionable since the most intransigent activists, in Brittany at least, refuse steadfastly to use French in any way. Such persons would thus not be captured by Beer’s survey, a result that biases his findings in the direction of moderation. Indeed, such a bias is implicit throughout the entire study; Beer consistently eschews the analysis of class relations and local social conditions in favor of such general processes as modernization, social mobility, and individual identity crises. What ultimately emerges, therefore, is a particularly unsatisfying form of psychological reductionism.

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ROBERT P. CLARK. *The Basques: The Franco Years and Beyond*. (Basque Series.) Reno: University of Nevada Press. 1979. Pp. xvii, 434. \$17.50.

Among the economically developed countries of the Western world, Spain is possibly the one that has made the least progress in the modernization of its internal structures. The present difficulties and obstacles hindering the establishment of democracy bear witness to this assertion. The search for a point of equilibrium between the need to maintain a reasonable degree of national coherence—with its inherent proclivity toward exaggerated centralism—and the recognition of the individual aspirations of the various units that make up the national state—with the inevitable corollary of its latent separatism—is the thorniest of these problems. It is in this area that Robert P. Clark did his research, and his work confronts the most complex of its aspects, that of the Basques.

As some authors—M. da Silva, Linz, and others—have pointed out, the Basques do not fit easily into the established patterns of nationalism. The force of their obscure ethnic and linguistic origins survives in an advanced and highly mobile society in spite of the repression they undoubtedly suffered during the forty years of Franco’s rule. Clark’s attention is directed mostly at this latter subject. He gives a detailed account of the ways in which Basques were discriminated against from the time of the Civil War to the beginnings of the post-Franco period. The Basques play the role of victims in his book. But the insistence on this truth may easily lead the reader to partial and even distorted conclusions. Throughout the book there is a tendency to present Basques and Spaniards as two totally separate categories, as if the Basques had not contributed to the making of Spain and as if all other Spaniards formed a cohesive group or alliance antagonistic to the Basques. One might also gain the impression that the Basques had been the only victims of repression. Culturally, Catalans and Galicians can offer an equally long list of injustices. Politically, Asturian miners or the workers of new industries surrounding Madrid were repressed as much as any Basque docker or steel worker. Economically, the peasants of Andalusia or Extremadura would be able to tell as sad a story as any Basque group, including the discrimination and exploitation they have had to suffer as emigrants in the Basque country—a subject that Clark bypasses altogether.

Otherwise, the book brings to the English reader a detailed and clear history of a people and a period that presents serious difficulties even for the scholar. The remote causes of Basque nationalism are not easy to grasp in our complex, postindustrial world. On the other hand, the events related are too recent to gain a wide historical perspective. Clark provides some enlightened guidance in both directions. Only his self-confessed Basquism distorts the final image offered to the reader.

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WILLIAM A. CHRISTIAN, JR. *Local Religion in Sixteenth-Century Spain*. Princeton: Princeton University Press. 1981. Pp. viii, 283. \$18.50.

From the perspective of field work and residence in Spain today and the discipline of ethnography, William A. Christian, Jr., provides historians of Spain’s Golden Age with fresh insights into past local religious beliefs and practices. The author used responses to questions 51 and 52 from the printed questionnaire that Philip II’s chroniclers sent out to towns and villages almost entirely within the five

central provinces of New Castile between 1575 and 1580. The history of Spain's greatness that was to result from these inquiries never materialized, but, as readers of Noel Salomon's work will know, a rich quarry of primary data did. Christian's final sections, "Sources and Abbreviations" and "Notes" (pp. 223-64), indicate the depth of his research.

The answers to these two queries enabled contemporaries—and the author—to discover the locations and rationales of innumerable local relics, shrines, and images that served as intercessors with God on behalf of the affected communities and, with vows, as indefinite protectors against future calamities. These also often entailed holy days at a microscopically local level that, when added to the general number of such occasions Catholicism offered, sometimes caused work problems at, for instance, critical harvest times.

Vows to a relic, shrine, or image usually resulted from the conjunction of an actual or potential threat from nature and a sign, perceived by those involved as mandating such propitiation. Devotional objects were at some distance from villages—out in nature, as it were. The local saints, formalized as "patrons" in 1630 by papal act, were "specialists" as compared with "generalists" such as Jesus, Mary, and Anne; worship of the saints belonging to the latter category increased steadily in this period, especially of Mary, who appears to have been unconnected with Trent. Interestingly, Christian describes a time of comparative prosperity; yet it, too, was marked by numerous natural afflictions connected with weather, locusts, and so forth, and an ever-growing tax burden. Thus, early in this era the Catholic kings' reigns were nostalgically regarded as the true Golden Age. Students from other perspectives have already remarked on this attitude.

Devotion to these "helper" saints was occasionally revalidated by successful manifestations of their ability to intercede, protect, and deflect perceived harm from the community. As the author sees it, this panoply of local, divine "advocates" mirrored the increasing legalistic and bureaucratic character of imperial Spain on which—again from other perspectives—historians have commented.

Despite a disclaimer that Inquisition materials are not very helpful for the researcher studying this field, Christian uses such data to supplement his work. Understandably, there were problems of both fraud and blasphemy involved in these matters, which were very much the Inquisition's concern. A growing body of scholarship has shown that the Holy Office played a very substantial role in controlling and indeed developing social behavior in addition to its more publicized functions. In general, the church had to be concerned with verifying the miraculous—especially in the age of Protestant and reformist challenge that Christian explores.

The era also saw the proliferation of religious brotherhoods; Christian's apt term for them is "salvation cooperatives" (p. 142). To some degree they, like local, popular faith in general, expressed concretely the need for local and regional cohesion through religion, all of which frequently also cut across customary barriers of wealth and class. The only true city responding to the questionnaire was Toledo, and on that basis the author observes that its "significant sacred geography . . . was not in nature but in society" (p. 149). Perhaps an art historian would like to compare that arresting remark with El Greco's "View of Toledo." In any case, the greater extent of class- and neighborhood-based devotions alongside all-city ones reflected the rural "specialist-generalist" pantheon. In both city and village devotions, Philip II himself participated, giving royal sanction to all such practices.

Christian notes that as images became perceived as ever more miraculous they also became more and more artistically realistic. In fact, he proposes that human and "miraculous" behavior also steadily merged; the sixteenth century saw the proliferation of flagellant cults, too—a reflection surely of heightened religiosity at all levels. The calamitous seventeenth century, however, saw the decline and disappearance of this kind of behavior, which fact would repay further investigation. Perhaps that was connected with Mary's ascension as prime object of such devotion; in the author's view, this represented the triumph of a benevolent, positive "helper" as the notion of an angry deity diminished. The richness and suggestiveness of his perspectives and judgments commend this study to any interested reader.

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JAMES C. RILEY. *International Government Finance and the Amsterdam Capital Market, 1740-1815*. New York: Cambridge University Press. 1980. Pp. xi, 365. \$32.50.

This useful volume contains an account of the development of public credit facilities in the Dutch Republic, an analysis of the role of foreign borrowing in the economic and political life of eighteenth-century Europe, a state-by-state account of government borrowing strategies, an account of the decline of the Amsterdam capital market, and an attempt to assess the impact of foreign lending on the well-being of the Dutch economy.

James C. Riley's matter-of-fact, almost clinical tone, his sometimes infelicitous prose, and the reputation of financial history for dryness, may act to limit the book's readership. This would be unfortunate, for Riley's broad approach to his subject infuses it with a new appeal. For example, although

nearly all borrowers had nonproductive purposes (mainly warfare) in mind for their Dutch funds, Riley demonstrates how certain states succeeded in gaining financial leverage from their borrowing activities, while for others the availability of Dutch capital simply served to postpone the reorganization of their inefficient fiscal systems. He also shows that the terms made available to even the least creditworthy states proved to be highly disadvantageous to the Dutch creditors. Even before the annulments and defaults of the Napoleonic era, Riley concludes, the debtor states had succeeded in turning the Dutch creditors into voluntary taxpayers!

How could the capital market have served the interests of Dutch savers so poorly? One might ask in return why modern capital markets suffer from the same defect, but Riley approaches the subject by detailing the workings of the bankers, brokers, and commission agents, whose self-interests tended to sacrifice the interests of the investors. Ultimately the author is led to the investors themselves, although they remain frustratingly elusive. Riley argues that their high propensity to save, together with disinvestment from declining sectors of the domestic economy, kept the volume of capital export high without much regard to the objectively disadvantageous terms available.

Ever since 1815 Dutch reformers and historians have been critical of their *rentiers*. Why had capital not been invested in domestic productive activities? Why had investors blindly accepted the poor terms offered by their bankers? Why, indeed, did they save so much for so little return? Riley investigates these matters, but it is here that his suggested answers are most likely to meet with skepticism or rejection. He favors the invocation of an economic compulsion that is never very satisfactorily explained. The *rentiers* faced an "imperative" to save; the broader environment rendered any Dutch action impotent to alter trends; it proved impossible to "find a policy whose short-term disadvantages were not excessive." Meanwhile, investors lost nearly half of the capital placed in the Amsterdam market.

This volume is easily the most comprehensive book on the Amsterdam capital market to exist in any language; it is wide-ranging in scope and very well documented. It also raises as many questions as answers.

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JAN LINDEGREN. *Utskrivning och utsugning: Produktion och reproduktion i Bygdeå, 1620–1640* [Conscription and Exploitation: Production and Reproduction in the

Parish of Bygdeå, 1620–40]. (Studia Historica Upsaliensis, number 117.) Uppsala: Acta Universitatis Upsaliensis; distributed by Almqvist and Wiksell, Stockholm. 1980. Pp. 325. 130 KR.

A research project, called "The Military State and Peasant Society," proposes an examination of the impact of war and a military state on a peasant (farmer) society, stressing exploitation through taxation and conscription. If Jan Lindegren's monograph is a good example of the project's intent, the assumptions must be re-evaluated, for the study misses its objectives, adding little information on the breakdown of the "feudal" and the creation of a new society or the effects of war on the parish itself. Having delivered this cannonade it is best that I review the book.

During the two decades from 1620 to 1640, Sweden warred continually: with Russia, against Poland, and in Germany after 1630. To wage war and create an empire Gustav II Adolph needed money, supplies, and conscripts; taxes and manpower from the peasants and officers from the nobility supplied these needs at cruel cost. Bygdeå, a small parish in northern Sweden with 2,000 inhabitants, furnished 300 conscripts, of whom 215 died, from a total male population of 800. Taxes rose but in less devastating fashion, a nominal 10 percent in twenty years, although money payments were difficult to find. Mostly the specie came from sales of butter, tar, and forest products; grain went for tax payments to be sold by the authorities or used as supplies for the military. Prices did not vary appreciably during the twenty years, despite two disastrous harvests in 1633–34.

Overall, the study starts with assumptions, methods, and sources and then moves to the socioeconomic structure in Bygdeå. The latter half examines conscription, taxation, and production. Graphs, diagrams, and tables abound, although the index and bibliography are less useful than expected in a monograph.

The work alleges exploitation and class struggle in the frame of Marxist materialism, a flawed assumption in view of Marx's ideology of the state and its role. The state was the exploiter through officials elected by the free farmers, excluding the parish pastor, himself a state appointee. There was no nobility, and the local merchants were less rapacious than in more settled regions and did not play a role in the collection of taxes or in conscription.

Production and well-being (except for famine years) improved, Lindegren states, but the yields of individual farms and totals necessary to prove these changes are lacking. The two decades saw an increase in money products to pay taxes and meet other demands, but what permitted this increase

and did it persist? In addition, mortality rates, especially the effects of the famine years, deserve some attention. The increase in the number of children and households creates a puzzle because with nearly 300 conscripts at war and over 200 dead, who manned the households and sired the children? Figures on increases in production disappear in an attempt to calculate calorie production and consumption. An assumption of a constant 1 percent surplus raises serious arithmetical and historical questions, for there is mention of variations, especially in money crops and losses in famine years, and the state did not secure all the surplus as theorized.

Debate will ensue—already it has begun in Sweden—about the work and its analysis, methods, and conclusions; undoubtedly this review will have its opponents. On the positive side, Lindegren shows the rich resources of information available, analyzes possibilities of model building, and, to a limited extent, describes a society that bore sorrow and pain and suffered through troubles caused by the elements and a state of war.

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ANN-SOFIE KÄLVEMARK. *More Children of Better Quality? Aspects on Swedish Population Policy in the 1930s*. (Studia Historica Upsaliensia, number 115.) Uppsala: Acta Universitatis Upsaliensis; distributed by Almqvist and Wiksell, Stockholm. 1980. Pp. 160. 63 KR.

In the 1930s, when demographers predicted Sweden's population decline, Gunnar Myrdal and Alva Myrdal persuaded fellow Social Democrats to abandon their neo-Malthusian traditions and tie population policies to the emerging social-welfare state. Did the rise and stability of Sweden's population in the 1940s mark the success of these policies? In *More Children of Better Quality?* Ann-Sofie Källemark of Uppsala evaluates the effectiveness of three such programs: marriage loans to selected young couples; maternity relief for poor women; and better housing for poor, large families. Reviewing Sweden's demographic history in the twentieth century and the population debate of the 1930s in a broad European context, she makes an important point: research should distinguish between population policies designed to increase the quantity or quality of a nation's people—or both of these—and social policies designed for the benefit of individuals. Since the effects of a good social program can hardly be discerned as different from those of a eutheic population policy, it is the motive behind implementation that provides the clue.

Carefully using statistical, ministerial, parish, and other archives, the author measures the impact of these programs regionally and nationally. Swedes clearly had a range of family behavior patterns, varying with class and provincial traditions as well as with their sense of the future. In evaluating each program, therefore, Källemark is sensitive to the values of the population the designers thought they would reach and makes valid selections of cohorts in different communities accordingly. She studies in depth 307 marriage-loan couples in Stockholm in 1942, a random sample of maternity-relief recipients in two Uppsala parishes compared to a control group, and 192 working-class families in the textile town of Borås along with a control group not occupying government-provided housing. These community studies have appropriate longitudinal dimensions and are enriched by subjective testimony gathered in government reports.

The subject abounds in irony. Marriage-loan couples chosen for their economic prudence had few children, while the other two groups contributed to Sweden's natural increase largely with unplanned pregnancies. Although both maternity relief and improved housing are termed qualitative successes, having met the planners' social intentions, the case of maternity relief is difficult to assess. The author believes that it was "in many ways a radical reform" (p. 96), but largely because giving even small sums to women alone violated the tradition that men control the purse strings. Since the relief was to soften the impact of a restrictive abortion law, however, its context, at least, remained pronatalist. In the study of the Borås workers with their increased rooms and facilities, improved family cohesion, and much lower rates of migration, the social impact of the program is clear. Moreover, insight into their social status, sensitivities, and socialist loyalties enriches the picture. Altogether, this is a gem of demographic history with important ethical as well as scholarly implications.

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ANTHONY F. UPTON. *The Finnish Revolution, 1917-1918*. (Nordic Series, number 3.) Minneapolis: University of Minnesota Press. 1980. Pp. 608. \$39.50.

Little more than a month after achieving recognition by the Soviet regime of its independence, Finland was plunged into a bitter civil war. How this tragedy came about is the central theme of Anthony F. Upton's book. The civil war, its international ramifications, and the aims and activities of the two sides, White and Red, is dealt with in the second half of the book. The first two hundred pages show how the social and political convulsions



of the revolution in the Russian empire affected Finland and caused the two sides to drift apart.

The title of this book suggests more than it explains. Finland was indeed caught up in the whirlpool of revolution brought about by the cataclysm of war; whether a revolution occurred in Finland itself is a matter for debate. The Finnish socialists were reluctant revolutionaries, to say the least. Their seizure of power in January 1918 was a defensive gesture rather than a determined bid for proletarian power and was portrayed by the Reds as a defense of democracy against a reactionary White government. As Upton rightly notes, there was a distinct lack of revolutionary fervor or commitment, and almost all the leaders of the labor movement sensed defeat from the outset. The real revolutionaries were to be found in the Red Guards, and it is one of the many fine achievements of this book that the role of these men is given due attention.

In spite of its length, this is an extremely readable book. Upton has drawn upon a wide range of archival and secondary material to re-create the events of 1917–18 in a way that no other historian has done. He is not afraid to pass judgment on the actions and ideas of the leading protagonists, and he is sometimes scathing in his criticism. A number of cherished myths of Finnish historiography are demolished, and certain features about which Finnish historians would prefer to keep silent, such as the widespread Russophobia that rose to an ugly crescendo during the civil war, are given firm but fair treatment. Upton has made good use of Russian material to illustrate the relationship between the representatives of the soldiers and sailors in Finland and the Finns, and he is at pains to show that, far from being a Bolshevik-inspired plot, the seizure of power was a muddled last attempt by democratic socialists to save something from a situation of impending civil war. The Finnish revolution that Upton portrays was a joyless and dour affair, and, in comparison with what the Bolsheviks were about, it can hardly be described as a revolution at all. It is in fact something of an oddity in the pattern of upheavals that ran through Europe between 1917 and 1920. Upton's authoritative interpretation is at times controversial, but it is solidly based on meticulous research, as even his most obdurate Finnish critics will have to acknowledge. This is a full and authoritative study that is destined to remain the standard work on the subject for many years to come.

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DAVID C. STEINMETZ. *Luther and Staupitz: An Essay in the Intellectual Origins of the Protestant Reformation.* (Duke

Monographs in Medieval and Renaissance Studies, number 4.) Durham: Duke University Press. 1980. Pp. x, 149. \$16.75.

David C. Steinmetz has written a thoroughly illuminating book whose subtitle does justice to its substance. In a comparatively short compass he has produced a lucid and nuanced analysis not only of the relationship between Staupitz and Luther but also of the main sources of Luther's theology. At the same time he has dealt briefly and convincingly with the accumulation of academic theories about both; although every fresh scholar is like Bernard of Chartres's dwarf sitting on the shoulders of giants, the legacy of the past can also obscure one's vision and divert it into unprofitable disputes over opposing theories.

Steinmetz fairmindedly surveys the main interpretations both of Staupitz's influence on Luther and their common theological influences. That includes once again the candidature of Gregory of Rimini for the main transmitter of late medieval Augustinian theology and the role of late medieval nominalism. In both cases he shows, in my view convincingly, that there is no evidence for the influence of either on the formation of Staupitz's or Luther's outlook. Staupitz not only did not cite Gregory of Rimini but also, as revealed in Steinmetz's exposition, there was in him nothing of Gregory of Rimini's doctrines of grace and predestination. As Steinmetz says, most of Staupitz's positions were those of an older Augustinian theology taken and developed from Augustine himself. There is perhaps indirect evidence of Gregory's influence in the return to Augustine's own writings that, according to Father Trapp, was one of the characteristics of the Augustinian order in the fourteenth century. In Luther's case, Steinmetz pertinently asks why, if there was an Augustinian doctrine of grace adopted by the entire Augustinian order from Gregory of Rimini, did Luther not know about it when studying at Erfurt and why had Staupitz to correct Luther's early theology?

That brings us to the relationship between Staupitz and Luther, which Steinmetz examines under four headings: biblical studies, justification, the mind of Paul, and religious ecstasy. A short review cannot do justice to the intricacy of Steinmetz's analysis. Only the last brief chapter on religious ecstasy is less than satisfying and rather inconclusive, doubtless because that aspect does not lend itself easily to analysis. But there, too, the same continuities in each thinker and their different approaches are apparent. Steinmetz identifies three ways in which Staupitz helped Luther theologically: the belief that penance begins with love of God, which was not, as Luther had been taught, the final stage of religious development; the belief that



one should distrust one's own moral energies; and the belief that true theology glorifies God rather than man. These three ideas were more than mere Augustinian platitudes, as Steinmetz conclusively shows. In Staupitz's case the centrality of love was expressed in the centrality of election, as the first grace to which all the other graces are subordinated. He thereby went beyond Augustine in reversing the order between justification and predestination, making the former the "fruit" of the latter—the moment when God becomes pleasing to man stems from the love that God has conferred upon man. Without God's love man was powerless to perform moral good, and the good that man performed under its influence was precisely the result of the recognition of his own sinfulness and impotence and the consequent glorification of God. Late medieval "nominalist" doctrines of congruous merit and predestination based on divine prescience of future human actions went out the window.

Luther followed Staupitz in that approach but with the fundamental difference that he made faith, not love, the foundation of justification and religious experience. That gave Luther's theology a radically different orientation from the outset, to be seen in his early expositions of the Psalms analyzed by Steinmetz. Faith was man's trust in God's covenant expressed in Christ; it directed the believer both to the future and to what was hidden in the present and could only be recognized by faith. Luther's theology thereby had a dynamic existential character even at the beginning, which distinguished it from that of Staupitz, whose influence was, as Luther himself used to declare, pastoral rather than doctrinal in bringing him to face theological issues. There could be no more enlightening introduction to their relationship than Steinmetz's book.

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ULRICH LANGE. *Die politischen Privilegien der schleswig-holsteinischen Stände, 1588–1675: Veränderung von Normen politischen Handelns*. (Quellen und Forschungen zur Geschichte Schleswig-Holsteins, number 75.) Neumünster: Karl Wachholtz Verlag. 1980. Pp. 346.

In this work Ulrich Lange traces the decline of five specific privileges once enjoyed by the estates of Schleswig-Holstein as examples of forms of political interaction and steps in the establishment of princely absolutism in the early modern European state. The privileges studied include two personal privileges of the nobility, their right to hunt and their personal legal status, and three political privileges, the estates' rights to elect the ruling princes

and consent to taxation and the nobles' monopoly of government office.

These privileges were first threatened in the early 1590s while the two corulers of the duchies, King Christian IV of Denmark and successive dukes of Holstein-Gottorp, were still minors. Although the estates attempted to stay out of the inheritance disputes that followed between the princely families and governmental regents, the previous consensus regarding the estates' privileges was questioned and made subject to future modification.

By 1614–15, however, Christian and Duke Johann Adolph had forced the estates to accept significant changes in most of these privileges. Nobles were forced, for example, to agree to restrictions on their hunting activities and to recognize their privileges no longer as a right but as a ducal favor. The establishment of primogeniture by the Holstein-Gottorp duke in 1608 made the estates' election of their princes obsolete and ducal confirmation of the estates' privileges meaningless.

Schleswig-Holstein princes destroyed the political influence of the estates completely by first making their consent to taxes superfluous and then eliminating it entirely. Christian IV's involvement in the Thirty Years' War and later conflicts with Sweden created financial needs the estates could not dispute. The estates demanded more accurate accounting procedures and the use of military force to eliminate overassessment and tax evasion but ultimately weakened their own position greatly by doing so. The final dissolution of the estates occurred in 1675 after King Frederik III and his successor, Christian V, successfully levied higher taxes than those agreed upon by the estates without opposition.

Lange provides several reasons for the decline of the estates and the slow development of princely absolutism in the duchies. War and general economic conditions were two basic factors. Advisers encouraged princes to follow absolutist policies. The estates as a whole did not always defend the privileges held by only a few. Absenteeism and unruly behavior plagued Landtag sessions.

Two minor points might have been considered by the author in this work. King Frederik III imposed primogeniture in his ducal possessions in 1650. Although Duke Johann Adolph's similar action in 1608 was more decisive, Frederik's move should probably have been mentioned to keep the record straight. The author might also have considered the impact of the Danish Royal Law of 1665 upon the duchies. Although applying only to Denmark, the law reflects the same attitude that moved its chief beneficiary, King Frederik III, and Christian V to then attack the estates' privileges so thoroughly in the duchies.

Nevertheless, Lange has presented an interesting,

insightful, and well-documented study of the duchies that also extends our knowledge of a central issue in the development of the early modern European state.

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RONALD TAYLOR. *Literature and Society in Germany, 1918-1945*. (Harvester Studies in Contemporary Literature and Culture, number 3.) Sussex: Harvester Press or Barnes and Noble, Totowa, N.J. 1980. Pp. xiii, 363. \$28.50.

There are a number of histories of German literature in print that deal in some detail with the period between the founding of the Weimar Republic and the end of World War II. Ronald Taylor's work purports to be different from many of these in that he eschews a purely literary approach to the subject by treating literature as a response to cultural and political reality. Artistic creativity, Taylor argues, "moves with the swell of political and social events" (p. xii). He tries, however, not to treat his subject too mechanistically, as if the writer, in responding to events, simply mirrors them and nothing more. A more accurate approach must consider that the writer reacts not only to reality as such, but also to internalized (and individualized) perceptions of it. Hence, for Taylor, artists have to be dealt with as unique personalities who maintain some unity in their work regardless of the course of worldly events. Although they obviously respond to reality, they also assimilate it in unique ways and organize it artistically for their own purposes.

With this approach as a guiding theme, Taylor proceeds to explore three decades of literary work in Germany after 1918. Unfortunately, the reciprocity of writer and society, which he wants to capture, is not always convincingly portrayed, partly because the book is organized too rigidly and predictably. Despite good intentions, Taylor often covers familiar ground in familiar ways, thereby missing the opportunity to rethink and reconceptualize how literature is usually talked about in works of this type. His first chapter, for example, describes the social and political background of the Weimar Republic, and the next discusses some of the cultural and intellectual forces whose origins predate World War I. Each of the next three chapters traces the development of a particular literary form. First comes an account of drama during the Weimar period, with special attention paid to Kaiser, Unruh, Piscator, and Brecht. Following this is a chapter on poetry, with some treatment given to the expressionists, dadaists, and working-class poets, although the focus is mainly on figures like Benn, George, Rilke—and once again, Brecht. Finally, in the long-

est chapter of the book, there is a discussion of the novel of the 1920s and early 1930s. The analysis here falls into five subtopics: the antiwar novel (Remarque, Renn), the prowar novel (Carossa, Jünger), the novel of postwar adjustment (Fallada, Döblin, Heinrich and Thomas Mann), the novel of generational conflict (Hesse, Wassermann), and the Austrian novel (Broch, Musil, Kafka). With this completed, Taylor introduces another chapter on social and political events, this time dealing with the years between 1933 and 1945. He then shifts to literature once again, first with a chapter on some of the major Nazi authors (Johst, Grimm, Kolbenheyer), then a chapter on the writers of the "inner emigration," and finally a chapter on those authors who went into exile. The book ends abruptly without a conclusion.

By the sheer number of names and works cited or discussed, this book comes closer to being a literary survey than an original contribution to the study of literature and society in Germany. Altogether, over two hundred authors are mentioned at least briefly (with approximately two dozen of them treated somewhat more thoroughly). Although this work is undoubtedly helpful for the overview it provides, it may seem too thin to those reasonably well acquainted with modern German literature, and perhaps not engaging enough to the general reader.

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FRED WEINSTEIN. *The Dynamics of Nazism: Leadership, Ideology, and the Holocaust*. (Studies in Social Discontinuity.) New York: Academic Press. 1980. Pp. xviii, 168. \$13.50.

In this short book, Fred Weinstein seeks to add to our understanding of Nazism. It is obvious, he writes, "that conventional approaches are inappropriate to an explanation of what we really want to know: the reasons, the intentions, the motives, the subjective strivings of people who ultimately proved responsible for such a unique and extraordinary degree of destructiveness" (p. xvi). To accomplish this goal, Weinstein proposes to focus on "problems of theory as well as of history and [on] attempts to explore the ways in which the basic concern of psychoanalysis (subjective strivings and perception) might be integrated with the basic concern of sociology (organized collective behavior)" (p. xi).

In four chapters, Weinstein deals largely with the nature of early intellectual and conservative support for National Socialism; the wider social context within which Nazism was allowed to develop; aspects of Hitler's leadership charisma; and, finally,

Nazi violence and the phenomenon of the Holocaust. Although there is the occasional striking observation—such as that the Nazis' obsession with modern (rational) technology remained in contrast to an ideologically fortified commitment to instinctual (nonrational) concepts rooted in "blood and soil" (pp. 128–29)—the book contains few new insights. In part this may be due to the not-so-happy marriage between sociopsychological-psychoanalytical theory and history that has resulted in a somewhat confusing methodology. The author's expertise in the first-mentioned twin disciplines may appear to be firm to the experts, but in history he is less surefooted. To some extent, the difficulties in historiography derive from the author's relative unfamiliarity with whatever happens to be the most appropriate source for the problem at hand. Hence, in the recurrent discussions of Nazi ideology, Eberhard Jäckel's and Barbara Miller Lane's authoritative analyses of National Socialist *Weltanschauung* have been ignored, and, on the Führer's progressive physiological deterioration, the more recent study by the noted Minneapolis psychiatrist, Leonard L. Heston, should have been consulted. Throughout, the author is fond of resorting to literature quoted elsewhere rather than going to the original source. A two-tier system of notes—some of them inordinately long and located at the bottom of pages as well as at the end of chapters—renders comprehensive study cumbersome; much of what is said in the notes should have been an integral part of the text. The organization of pertinent material at three separate levels makes one wonder about the author's ability to develop any theory cogently and coherently. The lack of a formal conclusion leaves the reader questioning why the book was written.

At the beginning, the author states that the purpose of his study is to supplant hitherto existing "unsuccessful" psychohistories in the field with more reliable ones (p. x). But if the new explanatory model runs the risk of being equally as hard to grasp as earlier ones, the student of history may come to question the validity of the entire (in this case) social-scientific approach in solving problems arising from Nazism.

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ERNST CHRISTIAN HELMREICH. *The German Churches under Hitler: Background, Struggle, and Epilogue*. Detroit: Wayne State University Press. 1979. Pp. 616. \$30.00.

Ernst Christian Helmreich is one of those accomplished American-born scholars of German descent named in David H. Pinkney's 1980 American Historical Association presidential address for their

contributions to the study of German history. Helmreich's earlier *Religious Education in German Schools* remains the definitive study of that subject. The present work is the labor of love of a mature scholar that was many years in the making. The author has brought to his task habits of careful research (there are more than one hundred pages of notes), deep knowledge of the subject, and balanced judgment, a quality needed by anyone attempting to venture into this field. The author deals with both the Catholic and the Protestant churches under Hitler and threads through the complexities of the subject with surety. Approximately one hundred pages take the relations of the churches with the state up to the advent of Hitler. New is the intricate account of the negotiations undertaken by Eugenio Pacelli, first as nuncio, then as cardinal secretary of state, for concordats with the *Länder* and the republic and his use of a German memorandum of 1920 to combat adoption of a federal school law. The core of the book, and that part most heavily based on documentary and archival sources, concerns the Hitler period. Helmreich makes as clear as is possible the complex relationships between the Reich church, the intact *Land* churches, and the German Christians and Confessing church. Knowing that the latter two were factions that could be found within *Land* churches and even within parishes gives some idea of the complexity of the situation, which was not only complex but also fluid, responding variously to the changing pressures, tactics, and policies of the state. Hitler finally gave up trying to do anything with the Protestants and adhered more and more to a policy of strict separation of church and state, but that did not end his troubles with the recalcitrant Confessing pastors and the courageous *Land* bishops Meiser and Wurm. The latter was remarkably outspoken and had much to do with putting a stop to the euthanasia program.

The Catholic church was better able to protect itself, since it was neither bedeviled by a German Christian faction sympathetic to Nazism nor plagued by problems of church organization and unity, and after 1933 the concordat provided some degree of protection. There were instances of heroism, but it was the finest hour of neither church. "Neither hierarchy nor pope ran away, none succumbed, none won crowns of martyrdom; all lived on to fight for their faith another day" (pp. 365–66)—an achievement of sorts, given the circumstances.

The book is heavy with detail, which adds, however, to its power, making it more than an account of the activities of the top leadership. The enumeration of constraints tells much about life under the Nazis. During the war Protestant losses of conscripted pastors and seminarians were appalling; paper supplies for publications were severely cur-

tailed; youth retreats were prohibited on the grounds that the time was needed for war-related activity; nearly all bells were confiscated, and the state even planned to seize the pipes from the organs.

Helmreich neither excuses nor demands the impossible of institutions or of human clay, but he makes clear that there were times when even mortals might have done better. He is the most morally persuasive author on the subject of the churches and the persecution of the Jews, rendering an account that would, I think, be recognizable to the Christian participants. Heroism and moral negligence or worse (sometimes in the same person) are given their due. The churches won no glorious battles but provided more resistance than other German institutions. Helmreich seems to suggest that, by facing their shortcomings of the Nazi era, the churches to some extent have and could still achieve an examination of conscience that would be to their benefit. He has provided a historical account that would aid in that task.

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JILL STEPHENSON. *The Nazi Organisation of Women*. London: Croom Helm or Barnes and Noble, Totowa, N.J. 1981. Pp. 246. \$22.50.

Jill Stephenson denies that her new book, *The Nazi Organisation of Women*, is "of the women's history genre" (p. 11). She is interested above all in the Nazi organization of women as a case study of the National Socialist movement. Having been accused by a reviewer of one of her earlier articles of doing no more than showing that Nazism was a male-dominated system, Stephenson hopes to contribute to our understanding of Nazism as a "movement which attracted and held the enthusiasm of a small minority of Germans who . . . attempted to impose their will on the majority" (p. 11). She has diligently searched the archives for the evidence that allows her to fashion the complicated story of the rise and demise of the Nazi women's organizations. If the narrative occasionally bogs down in detail and in the overuse of initials to refer to organizations and their subsections, it is nevertheless an important story. Because Stephenson so assiduously avoids drawing the overly simple conclusion that men dominated in Nazism, however, she emphasizes the relevance of the women's organizations' history for Nazism in general at the expense of putting together her conclusions into a comprehensive analysis of the impact of misogyny and male dominance on Nazi society.

This is not to say that Stephenson ignores the role that gender played in the history of the women's organizations. She points out that Nazi women in the

early years operated independently of the male leadership because they were considered so unimportant. But she does not explore the attitudes of Nazi women toward the question of separatism versus integration. Her own attitude is only too clear when she argues that well-educated German women "would hardly relish segregation in such a claustrophobically single-sex group" (p. 150).

When Stephenson, after analyzing the conflict-ridden process of centralization and stabilization, turns to the history of the women's groups under their eventual leader Gertrud Scholtz-Klink, she comes to the crux of her argument about the unpopularity of the Nazi regime. The *Frauenschaft* (Nazi women's group) failed to organize all German women largely because it took its shape from the middle-class women's organizations out of which it was formed, something that guaranteed that the working-class women whose own organizations had been destroyed would not be attracted to the Nazi replacements. The majority of German women remained indifferent and resisted passively the *Frauenschaft's* attempts to rouse them to ever greater sacrifices. As a result, the Nazi women's groups remained politically insignificant.

Stephenson is right in pointing out that the *Frauenschaft* had little power or significance, but what she ignores here is that the Nazi leadership did not really *want* a powerful women's organization. The fact that the women's organizations could not generate enthusiastic support does not mean that no Nazi organization could; to argue this way denies the central fact that National Socialism saw women as fundamentally different from men and refused to accept politics, broadly defined, as a proper sphere for women. Ultimately, then, Stephenson disavows the importance of her subject by presenting it as primarily a case study of Nazi organizations. The history of the Nazi women's organizations that Stephenson so ably details has a great deal to teach us about Nazi society, but her distaste for women's history makes her work more a straightforward account of ultimately insignificant organizations than the study of the profound impact of misogyny and male dominance on Nazi society that it might have been.

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WALTER LAQUEUR. *The Terrible Secret: An Investigation into the Suppression of Information about Hitler's "Final Solution."* London: Weidenfeld and Nicolson. 1980. Pp. 262. £8.95.

The "terrible secret" of what the Nazis called the "final solution" and of what we have come to call the "Holocaust"—a term the author thinks inappropriate—was never much of a secret at all,



Walter Laqueur tells us in this fascinating volume. News of the systematic killing of Jews began to leak out of the Soviet Union soon after SS *Einsatzgruppen* began fulfilling their grisly assignment in June 1941. Four months later, in October, a Swedish newspaper was already reporting indications of these murders, as well as of deportations of Jews from Germany and of the creation of ghetto installations in places like Vilna, Kaunas, and Bialystok. Information about the extermination camps, the first of which had begun operations in December 1941, was also available to the Allies by late summer 1942.

Through what channels was this terrible news transmitted to the Allies? How widely did they distribute it? Was it believed? Was it, as is often alleged, suppressed? These are the major questions to which Laqueur addresses himself. Some of the answers are surprising. For instance, some of the earliest hints the West received of deportations and ghettos actually appeared in local newspapers the Nazis were themselves publishing in the areas they were occupying, newspapers that Allied intelligence services were scrutinizing for all kinds of other information. Furthermore, the author finds the leading role in transmitting news of the "final solution" to the Allies to have been played by the Polish underground. He credits those in that underground with doing "what they could, usually at great risk and in difficult conditions" (p. 106) and concludes that a comparison with the French underground "would be by no means unfavorable for Poland" (p. 107). The mission from Warsaw to London in November 1942 of Jan Karski, one of the most important members of that underground, turned out to be vital to confirming to the West the fact of the "final solution." Karski's own detailed recollections of that mission are published in an appendix to this volume.

How did the Allies receive this information? At first, in large part, with disbelief. The discredited atrocity stories of World War I left even those most sympathetic to the Jews disinclined to believe the worst. Not every reaction was that innocent, however. Laqueur adduces convincing evidence that in some critical instances the Allies did indeed suppress the news of the extermination camps. British Ministry of Information (MOI) officials decided in July 1941 to use atrocity stories only sparingly in their domestic propaganda and to make no use at all of the reported persecutions of the Jews. Their calculations are instructive. The public, they feared, might consider victims so singled out as somehow deserving of their fate. Even more decisive in MOI reasoning, Laqueur finds, was "the widely reported prejudice in the British Community against Jews" (p. 92).

Although some of the questions Laqueur raises remain unanswered, either because they are un-

swerable or because the evidence is still not in (from the Vatican, for example), there is no longer any doubt that Allied governments were much more aware of this terrible secret than they were ever willing to admit.

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BURKHARD VAN SCHEWICK. *Die katholische Kirche und die Entstehung der Verfassungen in Westdeutschland, 1945–1950*. (Veröffentlichungen der Kommission für Zeitgeschichte, Forschungen, number 30.) Mainz: Matthias-Grünwald-Verlag. Pp. xxv, 156. DM 38.

This short dissertation seeks to rebut some of the criticisms of post-1945 German Catholicism advanced in the sprightly but skeptical account by Frederic Spotts, *The Churches and Politics in Germany*. Burkhard van Schewick's purpose is principally to outline the stance taken and the tactical maneuvers adopted by the Catholic hierarchy in West Germany during the months of debate over the new constitution that was finally agreed on in 1949. The key figure, whose activities are outlined fully, was Monsignor Wilhelm Böhler, the political advisor to Cardinal Frings of Cologne, who was also head of the German Bishops' Conference. As could be predicted by the appearance of this brief account in this series, van Schewick positively if somewhat defensively appraises Böhler and the Catholic position.

The German Catholic hierarchy after the war adopted the view that the evils of Nazism were attributable to the dangerous secularization of society and the falling away of man from God, the culmination of a process begun during the Reformation. The continuation of dictatorship in the eastern half of Germany, under Soviet instead of Nazi rule, convinced West German Catholics somewhat reluctantly to accept a democratic alternative, as long as its constitution clearly and irrevocably ensured a return to Christian principles. The argument basically came down to controversy over the desired guarantee of church privileges, especially church schools. The Catholics held that the Nazi seizure of control over education was evidence of the danger of state intervention. The future strength of the Catholic church necessitated separate schools, especially in situations where democratic elections might bring socialists to power, whose anti-clericalism was assumed to be as pernicious as that of the Nazis. Hence the need for constitutional guarantees.

Van Schewick points out the ambivalent relationship that Böhler held in respect to the various political forces jockeying for position in the new era. The prospective chancellor, Adenauer, firmly re-



jected the idea of restoring the old Center party purely to defend Catholic interests. But some bishops were not so reluctant, especially in Münster, where opposition to any compromise had been the motto since Bishop Galen's defiance of the Gestapo in 1941. Böhler and his superior, Frings, certainly sought to exploit the Catholic position but finally recognized the limits of what could be obtained. Equally, van Schewick outlines the controversy over the 1933 concordat. Some Catholics regarded this as a "bulwark" to be carried over automatically into the new era. Others recommended a tactical silence, lest controversy be stirred up over the extent to which this agreement with Hitler had led to the church's complicity in Nazi crimes. Van Schewick confirms the view adopted by Spotts that few among the Catholic hierarchy had any real political judgment, while Adenauer had too much to be a loyal follower of clerical commands. The 1949 constitution had to be a compromise, but, having adopted a position of ideological principle, some Catholics could only see it as a defeat.

Van Schewick seeks to show how much the Catholics contributed to the lengthy discussions of 1947-49. In view of the legacy of antidemocratic attitudes, clericalism, and sectional intrigue on the part of Catholics, and the parallel legacy of anticlericalism and iconoclasm among socialists, it is remarkable that the churches obtained as favorable a status as they did. Van Schewick admits that it was less strength of argument than political opportunism that led to this result. The public desire for a restoration of the pre-Nazi society and the need for West Germans to have an ideological justification for their anticommunism gave Catholicism a new chance. But it did not bring men nearer to God.

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IVAN SCOTT. *The Rise of the Italian State: A Study of Italian Politics during the Period of Unification*. (International Humanistic and Social Sciences Monograph Series.) Meerut: Sadhna Prakashan. 1980. Pp. v, 398, v. \$20.00.

Ivan Scott proposes to put Count Cavour in his proper place. Cavour contributed to Italian unification, but Victor Emmanuel and his ministers achieved it after his death. Their efforts deserve more attention than they have commonly received from historians caught up in "the internal actions of the national movement before 1861" (p. i). To redress the balance, Scott thoroughly chronicles the initiatives of the Italian king and his ministers, the great powers, and the pope, from Plombières to the conquest of Rome.

Some themes emerge from the detailed, widely researched narrative. The Italian campaign for uni-

fication proceeded, as it had before 1861, from three sources: the crown, the cabinet, and the radicals represented by Garibaldi and the Sinistra. Their strategies diverged, crossed, or combined in an incessant series of schemes that produced diplomatic initiatives, armed action, or just talk. Victor Emmanuel worked with some prime ministers and around others. When they acted together, royal influence put Venetia ahead of Rome. When ministers (like Ricasoli and Minghetti) acted on their own initiative, they concentrated on Rome, securing, for example, the withdrawal of French troops from the city under the September Conventions. But all the cabinets and the king rejected unilateral military action or overt support for Garibaldi's campaigns and relied on time and diplomacy to win Venetia and Rome. In any strategy they followed, they worked from a position of weakness. Success depended on the disposition of the powers, particularly France and the papacy. Italian governments generally understood these limits and practiced the art of the possible, Scott argues.

The record of Italian foreign policy after 1861 does not entirely fulfill Scott's claims for it. True, by 1870 the Italians possessed Venetia and Rome. But all the Italian designs and initiatives Scott describes accomplished very little; the completion of unification depended largely on leaders and events that the Italians did not command. Scott needs to show *how* exactly the cascade of Italian projects and actions advanced their cause if he wants to make Victor Emmanuel and company the peers of Cavour. Moreover, he needs to show why the later phases of the Risorgimento mattered so much if he wants to support his view that emphasis on events before 1861 is the "most persistent defect in Risorgimento historiography" (p. i).

Perhaps Scott means to show that the accumulation of seemingly fruitless actions directly prepared the eventual conquest of Venetia and Rome. If so, the presentation of evidence obscures the argument. The narrative itself mirrors the tangle of the epoch's diplomacy. A clear definition of what mattered, or an exact chronology, or a concluding chapter would have helped the reader through the detail. Moreover, more conscientious editing and the most cursory proofreading would have reduced the stylistic, grammatical, and printing errors that burden the text and at times obscure the argument.

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SPENCER DI SCALA. *Dilemmas of Italian Socialism: The Politics of Filippo Turati*. Amherst: University of Massachusetts Press. 1980. Pp. xii, 209. \$17.50.

The Italian Socialist party in the years before World War I is a classic case in the history of Euro-

pean socialism. Contemptuous of all the forms and instruments of bourgeois politics, the party's leaders learned through painful experience that it did make a difference to them which politicians were in power. Concerned to help the workers and attracted by electoral success, some socialists became reformists, only to be reviled by more orthodox Marxists. Differing degrees of reformism or compromise and of orthodoxy or intransigence led in turn to splits within both camps. The party's history was one of internal conflicts amplified by differences among parliamentary deputies, party hierarchy, and labor unions; by local and regional divisions; and by the clash of personalities. And there is special interest in the fact that out of these battles Benito Mussolini rose to prominence and that then, across the hiatus of Fascism, the heirs of the intransigent socialists evolved into Europe's most moderate, flexible, and successful Communist party.

There is something classic, too, about the figure who led the Socialist party through the repression and violence of the 1890s, strove to hold it together in face of temptations to cooperate with Giovanni Giolitti in the first decade of the new century, opposed imperialist war over Libya, and then lost his leadership in 1912. Filippo Turati was a middle-class intellectual but not a theoretician, a melancholic loner who proved a skilled organizer. He is the hero as well as the subject of Spencer Di Scala's book, which provides a concise and restrained account of the tactics and conflicts of these years, an account based largely on party publications and seen primarily through Turati's eyes. Even readers who emerge not altogether convinced that "Turati had the virtues of consistency, clarity, compassion, and, above all, a sense of the limits of political action" (p. 146)—such praise would seem to fit Turati's companion, Anna Kuliscioff, more closely—will find the book useful.

Di Scala considers the fissiparous maneuvering to be inherent in socialist theory, and the example of socialist parties elsewhere would seem to support that view. But this study might have probed more deeply into Turati's personality and the criteria he used in deciding which policies were both Marxist and practical; into the kind of organizations Turati built and the loyalty he won from certain groups of workers (such as the postal workers); into the peculiar perils of Italian parliamentarism and the isolation of a political class, which Turati himself seemed at times to share, as in his indifference to universal suffrage or his doctrinaire approach to the problems of the South. Gaetano Salvemini's famous attack on the socialists for holding such attitudes is presented effectively but in a discussion that concludes with the comment that Salvemini's position forty years later would be closer to Turati's. Such a discussion might instead have led into an important

assessment of the problems that Italy's social and political structure presented for socialists. Di Scala's sympathetic portrait of Turati's embattled leadership leaves plenty of room for further studies.

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SIMON SERFATY and LAWRENCE GRAY, editors. *The Italian Communist Party: Yesterday, Today, and Tomorrow*. (Contributions in Political Science, number 46.) Westport, Conn.: Greenwood Press. 1980. Pp. xii, 256. \$29.95.

According to Simon Serfaty and Lawrence Gray, this book aims at providing a "sophisticated introduction" to the history of the Italian Communist party (PCI) through essays contributed by scholars presently or formerly connected with the Johns Hopkins Center of Advanced International Studies in Bologna. Although the editors do not wholly achieve their goal, the book is a useful addition to the growing literature on the most important non-ruling Communist party.

The volume is divided into two sections dealing with the PCI's domestic strategy and its international relations. A good bibliographical essay completes the work.

On balance, the book's main thrust that enough countervailing domestic and international political factors exist to prevent the PCI from assuming dictatorial control if invited to share power is convincing; indeed, the contributors stress the PCI's democratic evolution. The discussions of the PCI's possibilities of obtaining a share in government and the implications this development would have for Italy, the United States, the USSR, and NATO are excellent treatments of complex issues. The contributors carefully define the terms of their arguments and are realistic in assessing the possible consequences. Their analyses demonstrate that PCI participation in a coalition government would raise questions not only for Washington but also for Moscow because of uncertainties in the strategic, domestic Italian, East European, and even Soviet situations.

Eurocommunism and the "historic compromise," the two most controversial policies of the PCI, receive a great deal of attention. The contributors agree that more relaxed international conditions and the native tradition of the Italian Communists made both possible. Federico Mancini, writing on "The Theoretical Roots of Italian Communism," stresses Antonio Gramsci's humanism, stating that Gramsci reluctantly accepted Leninism only because Fascism was triumphant (p. 14). Lawrence Gray argues that Gramsci influenced Palmiro To-

gliatti, who bowed to Stalinism but had in mind a gradualist strategy inspired by Gramsci. In 1944 Togliatti announced the famous "Svolta di Salerno," the first step in the PCI's transformation from a Leninist cadre organization to a mass party prudently working within the system to implement democratic reforms. Togliatti's successors have followed a similar policy ever since, cautiously defying or outwitting the Russians.

The PCI's transformation rightfully deserves applause, but should the party's policy of collaboration with practically anyone on the Italian political horizon be treated uncritically? The "Svolta" accepted the monarchy and Pietro Badoglio, a reactionary general. And what is to be said of the PCI's vote to insert the Concordat into the Italian constitution? One wonders on what basis Communist historians still attack pre-World War I Socialists for having cooperated with Giovanni Giolitti.

Pierre Hassner, author of the concluding essay, emphasizes the contradictions in present PCI domestic and international policy, criticizing the Italian Communists for attempting to reconcile the irreconcilable: Atlanticism and neutralism, European and Mediterranean orientations, EEC and Third World views. Claudio Terzi raises the question of what will happen if the PCI comes to power and its democratic reformist conception of attaining socialism is tested against the Soviet idea of a "qualitative jump," that is, the assumption of a political monopoly by one party ("dictatorship of the proletariat") and state ownership of all the means of production, both of which the PCI has renounced (pp. 132-33).

In sum, the book is most successful in dealing with the more recent problems of the PCI, Eurocommunism, the historic compromise, and international relations. A discussion of other issues along these lines would have further strengthened the book: the relevance of the Center-Left experience, the effects of the Italian Socialist party's new aggressiveness, and the relationship between the PCI's policies and the rise of terrorism.

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DEREK W. URWIN. *From Ploughshare to Ballotbox: The Politics of Agrarian Defence in Europe*. Oslo: Universitetsforlaget; distributed by Columbia University Press, New York. 1980. Pp. 368. \$30.00.

In spite of its title, this book is not concerned with political agrarianism as a whole but with one particular aspect of it—the rise and development of peasant parties, that is, of parties that seek to represent the interests and values of the peasantry alone, as distinct from parties that represent these interests

and values together with those of other economic and social groups. Hence, although the book does deal with all of Europe, it concentrates on those countries, principally in Scandinavia and Eastern Europe, where peasant parties have at one time or another been powerful and important. Derek W. Urwin asks how the emergence of such parties between the late nineteenth century and the outbreak of the Second World War was influenced by such factors as the agrarian structure; the experiences of industrialization, urbanization, and political democratization; and by what he calls "changes in the state landscape of the continent" (p. 12).

In attempting to deal with these problems, Urwin is unable to overcome the difficulties of using the concepts and procedures of political sociology to deal with a phenomenon that is very disparate and of which our knowledge is very unsystematic. He cannot establish typologies or observe correlations over a wide enough range, or reliably enough, to construct a theory of peasant parties. All he can do is to suggest partial correlations and possible typologies. Sometimes this approach can be enlightening, as when he points out that in countries where the peasantry is a minority of the population, peasant parties mainly function as interest-group representations, whereas in countries where the peasantry is a majority, such parties tend to be more ideologically "peasantist" (pp. 236-50). Sometimes the approach ends in triviality, as when Urwin announces, as one of the main conclusions of his book, that peasant parties may be distinguished according to whether they are individualistic or collective in nature, whether their goals are more or less explicit, and whether their tendency is to uphold or to challenge the status quo (pp. 272-73). Either way, Urwin's approach prevents him from giving any kind of general answer to the question he poses. He is fully aware that he is in no position to construct a theory of peasant politics and does not claim to do more than provide a basis for further investigation. But even for this purpose, some kind of unified explanation of peasant parties and their place in European society and politics is essential; to provide this, in the existing state of knowledge, one must be willing to make generalizations and find common elements even, if it is not blasphemous to say so, without rigorous proofs. This has, in fact, already been done by Heinz Gollwitzer, in the introduction to *Europäische Bauernparteien im 20. Jahrhundert* (1979), a recent collective work on European peasant politics to which Urwin does not refer. In general, Urwin's book is no substitute for that edited by Gollwitzer, for either its thought-provoking or informative value.

It is worth repeating, in conclusion, that peasant parties strictly so-called are only part, and in Central and Western Europe by far the smaller part, of

the phenomenon of political agrarianism as a whole. Urwin is perfectly well aware of this and is fully entitled to choose the subject he has. But by concentrating on a part, books such as Urwin's may unintentionally mislead us as to the nature of the whole. Thus in Urwin's book, and for that matter in Gollwitzer's as well, Catholic political parties in rural areas appear, quite correctly, as obstacles to the formation of peasant parties (pp. 13, 163, 196; see Gollwitzer, pp. 31–34). But in the history of political agrarianism as a whole, Catholic parties appear quite differently: as highly effective agents for the organization and representation of the peasantry in regions of strong Catholic belief, albeit in permanent alliance with other social groups and in such a way as to protect the institutional interests of the church. The fact that in the course of doing this they largely prevented the peasant parties from becoming powerful or important is in this context merely incidental. This is only one example of the differences that appear when one considers peasant parties in isolation and political agrarianism as a whole. Since the European peasantry and its politics are at the moment an academic growth area, we may expect more general works on the subject in the future. It is to be hoped that they will concentrate on the whole rather than on the parts.

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ALOIS MOSSER. *Die Industriektiengesellschaft in Österreich, 1880–1913: Versuch einer historischen Bilanz- und Betriebsanalyse*. (Studien zur Geschichte der Österreichisch-Ungarischen Monarchie, number 18.) Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1980. Pp. 338. DM 85.

Quantitative work in Habsburg economic history has so far focused on a statistical reconstruction of the monarchy's past at the macroeconomic level. The resulting pool of data on prices and on aggregate and sectoral output has helped to deepen our understanding of economic change in the final century of the monarchy's existence. In contrast, relatively little quantitative work has been done at the microeconomic level. Alois Mosser's *Die Industriektiengesellschaften in Österreich* represents an ambitious push in this direction. As Mosser notes, the techniques of balance sheet and profit and loss statement analysis used today in decisionmaking by most firms have not been used widely in historical research. His use of their techniques makes this study important from a methodological point of view.

According to 1862 legislation, modified in 1899,

firms organized on a joint-stock basis had to present their stockholders with annual balance sheet and profit and loss statements. Unlike Germany, official publication of these results was not required. An unofficial financial and economic publication, *Compass*, *Finanzielles Jahrbuch für Österreich-Ungarn*, however, regularly published the annual statements of joint-stock enterprise. Mosser concentrates on the results for seventy of these firms for which data availability is reasonably good. The sample covers a wide spectrum of sectors including mining, chemicals, textiles, sugar, machine building, and metal-lurgy.

Most of the book consists of a description and presentation of these data and their transformation into ratios and statistics well known to students of finance and financial analysis. Chapters 2, 3, and 4 define the various balance sheet and profit and loss categories, list the joint-stock firms, and discuss the relevant financial ratios. Chapter 5 deals with the representativeness of the sample among all joint-stock companies and of joint-stock companies among all industrial firms. In chapter 6 Mosser uses the *Compass* data to describe the structure of assets and liabilities and the profitability of the firms in his sample. The results are displayed in tabular and graphical form in a 117-page appendix.

The main task of integrating these results into broader issues of Habsburg economic history lies in the future. Mosser makes a start in chapter 7 by relating his findings to the debate on the character of late nineteenth-century development. Some scholars, for example, Eduard März and Herbert Matis, see the post-1873 economy evolving in two long waves—1873–96, a period of persistent stagnation, and 1896–1914, a period of rapid growth. David Good, Nachum Gross, and Richard Rudolph have challenged this view. They see these two periods as different in terms of the severity of cyclical activity and the trend of prices but the same in terms of the growth of output. According to Mosser's findings, the structure of enterprise did not display a sharp break in trend around the mid-1890s either. Instead he sees the *Gründerzeit* upswing (1867–73) and the Great Depression itself (1873–79) as initiating a phase of "radical structural change" (p. 73). This rupture paved the way for the evolution of large-scale industrial development in Austria that unfolded in a rather uniform fashion from the late 1870s to World War I.

The technical nature of this volume may scare off the nonspecialist. But for Habsburg specialists and business historians with a quantitative bent, Mosser has provided a valuable service by bringing to light a large amount of data.

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ADAM WANDRUSZKA and PETER URBANITSCH, editors. *Die Habsburgermonarchie, 1848–1918*. Volume 3, *Die Völker des Reiches*. In two parts. Vienna: Verlag der Österreichischen Akademie der Wissenschaften. 1980. Pp. xviii, 774; xii, 776–1,471. DM 232 the set.

Over two decades ago the Austrian Commission for the History of the Austro-Hungarian Monarchy was created, whose purpose was to produce a multi-volume work on the history of the empire from 1848 to 1918, to which Habsburg specialists from East and West would contribute. So far two excellent volumes have been published—*Die wirtschaftliche Entwicklung* (1973) and *Verwaltung und Rechtswesen* (1975). The present, or third, volume, *Die Völker des Reiches*, is in two parts, comprising together 1,471 pages. Subsequent volumes will deal with *Die Konfessionen*, *Aussenpolitik*, *Die bewaffnete Macht*, *Entwicklung des Verfassungslebens*, *Politische Parteien und Bewegungen*, *Sozial Strukturen*, and *Das kulturelle Leben*.

From the time the nationality question first caused major controversies in the early nineteenth century, nothing has distorted and colored our understanding of Habsburg history more than this subject. The relations among the ethnic groups for the most part have been presented in stark terms. The politicians paved the way for simplistic and emotional interpretations of the problem, and the historians followed the flag. Few attempts were made to understand the complexities of the issues involved. It is only in the last two decades that scholars in Europe and America have sought to overcome the prejudices, misconceptions, and distortions of the past, recognizing that the empire cannot be recreated and that scholarship, not nationalism, should motivate the specialist. The volume under review basically meets this criterion. It is a fine work. Anyone seriously interested in the Habsburg nationality question should consult it.

The present volume is divided into seventeen chapters, of which thirteen are devoted to the nationalities—Germans (written by Peter Urbanitsch, Berthold Sutter, and Friedrich Gottas); Hungarians (László Katus); Czechs (Jiří Kořalka and R. J. Crampton); Poles (Henryk Batowski); Rumanians (Keith Hitchins); Croats (Arnold Suppan); Serbs (Dimitrije Djordjevic); Slovaks (L'udovit Holotík); Slovenes (Janko Pleterski); Italians (Umberto Corsini); and Ruthenians, Jews, and other national and religious groups (all three chapters by Wolfdieter Bihl). The other four chapters consist of a brief, introductory chapter on Habsburg history from the tenth century to 1848 (Erich Zöllner), equal rights for the nationalities as a constitutional principle (Gerald Stourzh), Hungary's nationality laws and the problem of the Hungarian nation state (Ludwig Gogolák), and the general issue of the nationality

question in the Habsburg monarchy (Robert Kann). In addition, there is a brief section on Transylvania and Bukovina by Ștefan Pascu.

Understandably, some of the chapters are more effective than others in part at least because more space is devoted to some ethnic groups than others. For example, there are 377 pages allocated to the Germans and 174 to the Hungarians, but only 32 pages each to the Poles and Czechs, in contrast to 37 for the Slovenes, 40 each for the Italians and Rumanians, 68 for the Jews, and 107 for the Croats. Of course, many issues involving the nationalities are discussed in the chapters on the Germans and Hungarians. Yet perhaps a somewhat better distribution of space could have provided more information for the reader about the other nationalities and thus a more balanced picture of the condition of these people, who comprised over 55 percent of the population.

Although there is no absolute uniformity on the subjects covered in the chapters on the nationalities, most of the authors have analyzed a number of similar topics, which contributes to the strength of the volume. Thus, in addition to the normal contributions on general historical and political developments, excellent information is given on demography, religion, culture, economics, urbanization, class structure, education, and statistics. The sections on education are particularly interesting, in part because this subject has not received as much attention in the past as it deserves. The politician was the nationalist spokesman, but the citizenry was taught nationalism through the educational systems. As ever more schools were built in the empire, nationalism became correspondingly stronger. In Cisleithania Vienna hoped that the schools would enhance the monarchy's prestige and weaken the nationalist movements, whereas in Transleithania Budapest was determined to use the schools to Magyarize the Lands of St. Stephen. Both policies failed. The contrasts between the Austrian and Hungarian educational policies are very instructive.

The statistical tables are also most helpful. The Habsburg authorities kept excellent records after 1848. Yet only in the last two or three decades have scholars turned to this valuable source to check the subjective generalizations about Habsburg affairs that historians and others have made in the past. Much of this information is presented in convenient form in the 94 tables in this study. For example, the first table, prepared by Peter Urbanitsch, includes the population of the nineteen Cisleithanian provinces by nationality and "Umgangssprache" in both absolute terms and percentages for the Germans, Czechs, Poles, Ruthenians, Slovenes, Serbo-Croats, Italians, Rumanians, Magyars, and Jews for



the years 1851, 1880, 1890, 1900, and 1910. Thus at a glance the reader can see the complexity of this issue. Equally informative are the tables on religion, education, occupations, tax structures, and urbanization.

Special note should be made of the perceptive chapter by Gerald Stourzh on equal rights for the nationalities and Ludwig Gogolák's analysis of the nationality laws and educational policies in Hungary. There is also a large, vivid supplementary ethnographic map of the empire that reveals why it was impossible to draw acceptable ethnic boundaries after 1918.

The Austrian authorities are to be congratulated not only on the excellence of this volume but also on the scope of their undertaking. When the remaining volumes are completed, we should have a clearer understanding of Habsburg history, which will help answer the question—"Notwendiger Völkerverein" oder "Völkerkerker?"—posed in the introduction by Adam Wandruszka, who, together with Peter Urbanitsch, is the editor of this comprehensive work.

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HUGH SETON-WATSON and CHRISTOPHER SETON-WATSON.  
*The Making of a New Europe: R. W. Seton-Watson and the Last Years of Austria-Hungary.* Seattle: University of Washington Press. 1981. Pp. x, 458. \$50.00.

This is a study of the public career of the British historian Robert W. Seton-Watson (1879–1951), especially his role in influencing British official and public opinion to dismember the multinational Habsburg empire and replace it with national states. The authors, Hugh Seton-Watson and Christopher Seton-Watson, are his sons. Both are historians of distinction in their own right.

Seton-Watson was not a retiring ivory-tower scholar but an activist academic with a strong sense of mission. Born in London into a devoutly religious Scottish Presbyterian family, he made his influence felt far outside academe. After 1918 he became the patron saint, as it were, of the new states of Czechoslovakia, Yugoslavia, and, to some extent, greater Rumania. Freed from material worries by an inheritance from his father, a businessman who had amassed a comfortable fortune in India and England, Seton-Watson believed that this placed on him a "positive obligation to undertake work of an unremunerative kind, such as others who had to earn their own living could not attempt." He decided to devote himself to the promotion and defense of "unpopular and neglected causes."

Seton-Watson received a good humanistic educa-

tion at Winchester and Oxford. His interest lay in the study of history and its relevance to the great political issues of his day. Around the turn of the century, when he reached maturity, the issue that most engaged the interest of politically conscious Europeans was the mounting international tension, especially Anglo-German rivalry, which ultimately led to the outbreak of World War I. In 1903 Seton-Watson gave up his original intention of pursuing a legal career in Scotland and spent the next three years studying international affairs and foreign languages at the universities of Berlin, Paris, and Vienna.

Originally, Seton-Watson believed that Austria-Hungary was indispensable to the European balance of power as a buffer between Germany and Russia. His study of the internal problems of the Habsburg empire convinced him, however, that the monarchy could not fill this function, because it had tied itself inseparably to Germany by the Dual Alliance of 1879 in order to secure Germany's support for the Austro-Hungarian Ausgleich of 1867, thereby assuring a privileged position to the Germans in Austria and the Magyars in Hungary. In the plight of the empire's "subject peoples," Seton-Watson found an "unpopular and neglected cause" that he was ready to defend.

During World War I, Seton-Watson, together with Wickham Steed, the political editor of the *Times*, and Slavic exiles, tirelessly promoted the idea of dissolving the Habsburg monarchy. His principal organ was the weekly *New Europe*, which he founded together with Thomas G. Masaryk in 1916. But its editing by no means exhausted his great energy. He contributed to all promotional activities of the Austrian Slavic exiles designed to influence Allied policy, as well as to Allied propaganda designed to induce the Austrian Slavs to revolt. In the end, his efforts were successful. Britain, which was not an implacable enemy of Austria at the outset of the war, became firmly committed to its dismemberment by the war's end. The Habsburg monarchy broke up even without the necessity for the Allied armies to occupy Vienna and Budapest.

At the Paris peace conference Seton-Watson tried to influence his Slavic friends, now leaders of Austria's successor states, to moderate their demands. In this he was not notably successful. As so much of the Paris peace making, the "New Europe" of national states did not quite rise to its promise. Seton-Watson, however, never repudiated it or showed disappointment over his life's work. A thorough student of Central European affairs, he knew—unlike some facile observers—that the Habsburg empire, moribund in 1914, was dead beyond resurrection in 1918.

This is a very careful study. It is documented not only by reference to Seton-Watson's personal papers

but also by a large selection of documents in British, Austrian, Hungarian, Yugoslav, and Rumanian archives, as well as published literature in many tongues. The authors are as versatile linguists as their late father. Although they planned their book as a study of British policy and public opinion rather than a biography of their father, by providing the book with a good prologue and long epilogue, they do offer the reader a good survey of Robert W. Seton-Watson's remarkable life and accomplishments as well.

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DIMITRIJE DJORDJEVIC and STEPHEN FISCHER-GALATI. *The Balkan Revolutionary Tradition*. New York: Columbia University Press. 1981. Pp. xv, 271. \$20.00.

In 1965 Dimitrije Djordjevic published his well-known volume, *Revolutions nationales des peuples balkaniques, 1804-1914*. The present volume covers roughly four centuries of revolutionary events on approximately the same number of pages devoted to one century in the previous study. Obviously, the authors' treatment of the crucial nineteenth century is less detailed this time than it was in the 1965 volume, but the present work by Djordjevic and Stephen Fischer-Galati gives a much broader and more comprehensive picture of the development of the Balkan revolutionary tradition.

As one would expect from two well-known experts, the authors produced a factually correct study that shows clearly how and to what extent well-accepted national myths of the Balkan peoples differ from historical truth. Their approach is broad and comprehensive, and the authors successfully show how the various forms of revolutionary movements were born and transformed under the influence of both local conditions and outside stimuli. Yet the authors also manage to include in their work little-known details about, for example, the Seimeni revolt (p. 21), which is hardly even mentioned in other English-language works.

The first chapter, describing the *Pax Ottomanica* and Ottoman institutions, the framework in which the revolutions occurred, is a good short summary of a very complex problem. The next two chapters, devoted to the seventeenth and eighteenth centuries, show that revolutionary movements during these years were either simply jacqueries, not national uprisings, or were instigated by small groups of discontented individuals who were unable to mobilize peasant support. The fourth chapter, dealing with the early nineteenth century, begins with an excellent short summary of the international situation and ends with the important conclusion that by roughly 1830 local considerations were more im-

portant in shaping events than were the plans of either the Habsburgs or the Romanovs. The next two chapters, bringing the reader up to 1878, are packed with facts that are explained by skillful interpretive comments. The sixth chapter, ending with 1914, is the weakest in this reviewer's opinion. The interwar years received little attention, and the epilogue dealing with the Bolshevik and Balkan revolutions is just that, a long footnote ending the narrative.

The volume is remarkably free of typographical errors and is printed in easily readable type. The footnotes serve mainly to give a basic bibliography to those interested in doing additional readings. The index is not detailed enough to make this volume an easily usable reference work.

The general reader and even the Balkan specialist will not find another volume equal to the one under review and dealing with the same subject. To write a better study on Balkan revolutions would require a book of about twice the size. Given the excellence of the authors' work, it is annoying that some easily avoidable errors were not corrected. These include the omission of accents in Hungarian words, although they are supplied for those in all other languages. Hercegovina is misspelled consistently as are Zsitvatorok (p. 17) and Tugendbund (p. 142) and a few other names; Kállay was not an Austrian historian but a Hungarian diplomat and politician (p. 73); it is certainly incorrect to label the rule of the Fejérváry government a dictatorship (p. 185). Errors of this kind are regrettable, but they do not diminish the value of this excellent study.

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V. A. FEDOROV. *Krest'ianskoe dvizhenie v tsentral'noi Rossii, 1800-1860* [The Peasant Movement in Central Russia, 1800-60]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1980. Pp. 164. 1 r. 70 k.

The term *krest'ianskoe dvizhenie*, or "peasant movement," covers a variety of manifestations of rebellion and insubordination. In the scholarly literature on the peasant movement in nineteenth-century Russia, there are two prevailing modes, the statistical and the narrative. Adherents of the statistical mode are like participants in an Easter-egg hunt. They search through the sources to find as many instances as they can of *volneniia*—"upheavals" or "perturbations." The successful historian is the one who finds the most. (B. G. Litvak and his students have produced a data base that would lend itself to forms of statistical analysis more sophisticated than counting, but they have not attempted such an analysis.) The statistical mode suffers from the limitation that the quantity of recorded *volneniia* is

largely a function of the state of mind of the squires and officials who produced the written record; totals reflect their anxiety or complacency more surely than they register the extent of the peasant movement. The narrative mode, as practiced by I. I. Ignatovich and others, amounts to the serial description of particularly dramatic *volneniia*; the reader is morally edified but not much assisted in making analytical judgments. Both modes tend to magnify, and probably to exaggerate, the extent and intensity of the peasant movement.

V. A. Fedorov of Moscow University operates in both modes, aware of the limitations of each. His struggle against these limitations is largely in vain. He offers seven statistical tables, one of which is of real interest. Here Fedorov finds that of 793 *volneniia* for which the cause was recorded peasants were prompted to act in 288 instances by an increase in dues, and only in eight were they responding to outrages against individuals; this finding confirms the observations of Peter Kolchin and other historians. Still and all, Fedorov's statistical procedures are limited to counting. Most of the rest of the book consists of narrative, occasionally interspersed with Fedorov's commentary and sometimes amounting to a mere list of *volneniia*.

Methodologically conservative, *Krest'ianskoe dvizhenie* is nonetheless a work of real value. Its merits derive from Fedorov's massive erudition, tireless energy, and rich experience with the sources. For example, he offers masterful syntheses on the use of troops in recalcitrant villages and on the serfs' manipulation of legal norms. His section on the response to the bestowal of peasants upon the favorites of Paul I opens new perspectives on the master-serf relationship. He has uncovered cases of "lying out" and maroonage, forms of protest characteristic of the bondsmen of the Western hemisphere. Most important, Fedorov has shown the ways in which peasant communities could secure marginal gains despite their utter lack of rights or standing at law. Throughout the text there are striking points of detail.

The considerable merits of *Krest'ianskoe dvizhenie* derive from Fedorov's years of work on printed sources, in the central state archives, and, most notably, in the provincial (*oblastnye*) archives of each of the seven provinces around Moscow to which the book is devoted. One finishes the book wishing it were as long as Fedorov's 1974 monograph on the peasant economy of these same provinces. Fedorov has not transcended the limitations of the prevailing modes of treating the peasant movement, but he has come as close as space permits to exhausting the possibilities of these modes.

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N. M. IAKUPOV. *Revoliutsiia i mir: Soldatskie massy protiv imperialisticheskoi voiny, 1917-mart 1918 gg.* [Revolution and Peace: Soldiers Against the Imperialistic War, 1917-March 1918]. Moscow: Mysl'. 1980. Pp. 243. 90 k.

In the infertile garden of Soviet historiography of the revolution, N. M. Iakupov's book is a particularly unattractive weed. There is nothing new anyone can learn from this retelling of the story of the "Bolshevik struggle for peace" in 1917 and 1918. Iakupov recapitulates the same nonsense that we have heard through the decades. Once again we hear that Kerenskii and Kornilov cooperated against the revolution; that Trotsky betrayed the Bolshevik cause at Brest-Litovsk; that it was Zalkind, Markin, and Polivanov (that is, not Trotsky) who published the secret treaties in November 1917; and that Kaledin was initially successful against the new regime because of the help he had received from the Allies, and so forth.

Let us imagine a naive, unfortunate reader, whose entire knowledge of the Russian Revolution comes from Iakupov's book. Surely such a reader will soon begin to wonder. How could it be that, on the one hand, the Russian soldier was the best in the world and that on the company level the Russian army was better than the German, while, on the other hand, the Russian soldier did not believe in this war and wanted only peace? Even more remarkably, how could it be that the Bolsheviks opposed the war on principle, carried out an extensive propaganda campaign against it, but were not responsible for the disintegration of the army? If we believe Iakupov, the regiments in which the Bolsheviks were strong were the ones that best retained their discipline and combat readiness. It was the generals, the SRs, the Mensheviks, and the bourgeois politicians who were responsible for the collapse of discipline.

A historian who has even the slightest interest in constructing a believable version of events would recognize that one cannot have it both ways. One must give concessions. But Iakupov has no more interest in believability than those who construct the slogans pasted on the walls of Moscow praising socialist legality and offering "glory to work." Iakupov, like the authors of slogans, is interested not in the power of sentences to convince but in the magic of incantation. For him rationality imposes no limits.

The sources of the impulse to make contradictory assertions are easy to understand. On the one hand, Iakupov has to safeguard the legitimacy of the October Revolution by pretending that the Bolsheviks were always right and virtuous and that their enemies—the generals, the Allies, the Germans, and

the "petit bourgeois politicians" (as Iakupov calls the Socialist Revolutionaries and Mensheviks)—were always wicked and wrong. On the other hand, it is necessary for him to enhance Soviet patriotism by projecting it into the past. The difficulty, of course, is that Lenin had a different concept of patriotism than Iakupov (or Brezhnev, for that matter). The Bolsheviks of 1917, unlike Iakupov, truly did not care whether Germany or Russia won the war.

It is, however, hard to see the source of his virulent anti-Trotskyism. One, of course, would not ask a Soviet historian to do justice to Trotsky's accomplishment, after a scant sixty-three years of the revolution. But why cannot Iakupov simply ignore him? Why is it necessary to repeat the Stalinist lies that Trotsky intentionally hurt the revolution by recognizing the Ukrainian rada, by not accepting German terms, by negotiating with the Allies about a possible resumption of aid? Perhaps it is a matter of conservatism. Iakupov would not want to deviate one iota from the verities of his childhood. Indeed, there is nothing in this book that could not have appeared in Stalin's lifetime. It is depressing to see that in 1980 still not the slightest concession is given to truth.

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BEATRICE FARNSWORTH. *Aleksandra Kollontai: Socialism, Feminism, and the Bolshevik Revolution*. Stanford: Stanford University Press. 1980. Pp. xiv, 432. \$28.50.

Aleksandra Kollontai is best known for her attempt to develop a form of socialism receptive to the special needs of women. Personal needs, Beatrice Farnsworth argues, caused Kollontai to opt for socialism over feminism and to retain a sensitivity to psychological issues. She then discusses Kollontai's conversion to Bolshevism, her activities as a member of the Central Committee, as director of *Zhenotdel* (the women's section of the party), and as opponent of the 1926 marriage law that allowed alimony for *de facto* (common law) marriages. The romantic drama of Kollontai's personal life is sensitively treated, her solution to the "woman question" detailed, and the milieu in which *Zhenotdel* activists operated in the 1920s is described. Setting Kollontai in the larger context of Bolshevik politics and Soviet history, Farnsworth views her as a "socialist activist pursuing libertarian goals in an increasingly authoritarian society" (p. xiii).

Kollontai's socialist feminism has already been described by Barbara Clements and others. The contribution of Farnsworth's study is to shed new

light on prominent socialist women and on intra-Bolshevik party dynamics. Kollontai's views are set off against those of Clara Zetkin (her mentor and then her opponent) and Rosa Luxemburg, who, as Farnsworth notes, enjoys a more democratic reputation but was never the member of a party in power. Bolshevik women such as Ludmilla Stal' and Vera Golubeva emerge from obscurity, and the different fates of Varvara Iakovleva, Elena Stasova ("a more congenial token," [p. 120]), and Sonia Smidovich (later head of *Zhenotdel*) in the party hierarchy are analyzed. The only two women, Kollontai and Angelica Balabanoff, who dared challenge the all-male leadership not only lacked the usual male mentor, but felt themselves superior, not merely equal, to their male comrades (pp. 281-83).

Particularly valuable is Farnsworth's discussion of Kollontai's role in the "Workers' Opposition." Showing that the latter was not, as is usually assumed, a monolithic group, she explains Kollontai's reasons for association with it and her emergence as leader. Although Kollontai advocated workers' control over production, she opposed the labor unions' demand for political autonomy and for dissolution of the labor armies. Conflicts between the labor unions and *Zhenotdel* concerning jurisdiction over working women plus desire to counter suspicion of female separatism led her to insist that all organizations be subordinate to the party. Democratization of the party itself, weakening of bureaucratic authoritarianism, and scope for worker spontaneity were to her the key issues; she defied Lenin and went before the Comintern to argue her case.

Farnsworth has captured Kollontai's emotional intensity, describing her as a typical left communist "doctrinaire and impatient" (p. 70). But her own evidence of Kollontai's zealotry, authoritarianism, and even ruthlessness contradicts her idealization of Kollontai as a libertarian. Advocating that exhausted Russia create an international revolutionary army, she opposed the treaty of Brest-Litovsk. Like the French revolutionaries before her, she saw the Civil War as "a boon to her radical goals" (p. 129). In "an early and more or less benign version of the Party purge" (p. 231) she urged expulsion of nonproletarian elements. Most important, conscript labor (the labor armies) became the cornerstone of her image of women's liberation, the key to a restructuring of living patterns along communal lines, establishing woman's role as provider, and breaking the old psychology of dependence on men. Willing to "force them to be free," she was intolerant of those who did not share her vision of women's liberation, and she dismissed the hardships her solution would have imposed on them as only temporary.

This is a rich, well-written, but somewhat partisan study that labels disagreement with Kollontai



as conservative, backward, or petit bourgeois. Still, it joins Barbara Clements's *Bolshevik Feminist* as required reading for students of Soviet history, women's history, and socialism.

BERNICE GLATZER ROSENTHAL  
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ALAIN BESANÇON. *The Rise of the Gulag: Intellectual Origins of Leninism*. Translated by SARAH MATTHEWS. New York: Continuum. 1981. Pp. 329. \$19.50.

MARC FERRO. *October 1917: A Social History of the Russian Revolution*. Translated by NORMAN STONE. Boston: Routledge and Kegan Paul. 1980. Pp. xiv, 345. \$35.00.

We have here, in fluent English translation, the recent works of two of France's leading historians of Russia. Both books bristle with provocative interpretative ideas, and both deserve to find large audiences. But I suspect that it is Marc Ferro's that will continue for many years to be an essential item in any bibliography in its field.

*October 1917: A Social History of the Russian Revolution* is the sequel to *The Russian Revolution of 1917: The Fall of Tsarism and the Origins of Bolshevik Power*, the French edition of which was published in 1967. That volume carried the revolution through June, while Ferro's new work begins approximately with the July crisis and ends with the October uprising. It is indeed a social history, but no less a political history, as is appropriate in dealing with a time when social upheaval and politics interacted so directly and violently. Ferro is at his best in describing the lower-level sociopolitical institutions such as the workers' committees and (urban) district soviets as bodies that reflected grass-roots radicalism and tended to press ahead of the better-known, higher-level bodies, such as the Petrograd Soviet and even the Bolshevik party. His research draws on recent Soviet studies and his own probings of archives in the USSR, from which he has extracted both statistical and anecdotal evidence.

Ferro attempts, with considerable persuasiveness, to displace the interpretation, for years highly influential in the United States, that sees the October Revolution mainly as a conspiracy of the Bolsheviks that capitalized on transitory and accidental circumstances. Instead, he stresses the breadth and depth of the authentic social revolution from below, with its natural proclivity for Bolshevism and even anarchism.

The disintegrative impact of this mass movement he sees, perhaps more than any other writer, as being exacerbated by the conduct of the bourgeoisie and in particular the Constitutional Democratic party, through its attempts to meet the revolution

with lockouts and various counterproductive forms of right-wing propaganda. Ferro offers some fresh and valuable evidence in this direction, but he may have gone too far in asserting that in the distribution of antisemitic propaganda "the Kadet leaders seemed to be pulling the strings" (p. 39). A footnote concerning these matters offers no convincing evidence about the main Kadet leaders, who had so often been branded as "Jew-Kadets" in previous years.

Despite the great value of Ferro's research on the social revolution, his work is uneven in this area. The treatment of the peasants, written before the publication of Graeme J. Gill's excellent *Peasants and Government in the Russian Revolution*, is brief, relative to the scope and complexity of the topic, and not as revealing as Ferro's handling of the urban scene. The treatment of the bewilderingly diverse problem of the minority nationalities is a valiant attempt at analytical compression but can hardly provide even a synoptic narrative in a fairly short section of the book. Ferro's handling of this theme is perplexing in that he devotes so much attention to Finland, and especially the post-October developments in Soviet-Finnish relations, all the way to 1948. The same tendency to follow a theme of 1917 well past that year, and in highly cursory fashion, weakens his treatment of the question of women.

In emphasizing the importance of the revolution of the masses, Ferro does not belittle the importance of leadership, and his concise analysis of the October Revolution includes a penetrating presentation of Lenin's unique contribution. In his interpretation it is crucial that Lenin "wanted a breach between the Bolsheviks and other revolutionary organizations" (p. 258), and for this reason the armed insurrection was necessary. Even though Trotsky played a leading role in the direction of this uprising, Ferro stresses the difference between the outlook of the two Bolshevik leaders, even noting "mistrust" by Lenin of Trotsky's participation in "revolutionary legalism," which might have led to Lenin's submersion in some kind of socialist coalition.

Concerning Alain Besançon's work, the first point to be made is that it does not deal explicitly with police or labor camps. The allusion to Gulag in the title apparently is the work of the American publishers of a book that was originally called *Les origines intellectuelles du Léninisme*. It is an excursion over a vast landscape, including the gnostics, modern French, German, and Russian intellectual history. Besançon offers a variety of learned and imaginative observations, but some of his central points are more provocative than persuasive. He conceives "ideology" as a quite special phenomenon that is found only in "Hitlerism and Leninism" (p. 52). The writer is no doubt entitled to establish definitions, but, having done this, should adhere to them. Besançon does not. Although he explicitly deter-



mines that Marxism (the ideas of Karl Marx) "avoids ideology only because of the Faustian temperament of its creator" (p. 44), Besançon goes on to refer to ideology in the context of Russian religious thought, populism, Chernyshevsky, and the July Monarchy in France. This sounds much more like the loose, ordinary use of the word than any tightly limited conception.

Leaving aside this possible inconsistency, one can pay tribute to Besançon's agility in finding sources of Leninism in both conventional places, such as the lives of Nechaev and Tkachev, and unconventional ones, such as the gnostics and Slavophiles. The emphasis on the importance of the former is at least controversial. Besançon himself sharply distinguishes the religious basis of gnosticism from Leninism, and one wonders if the presence of dualism and the three times (past, present, future) to which this study repeatedly refers in both gnosticism and Leninism is as revealing as Besançon maintains. As for dogmatism ("the 'eureka' of ideology"), which he persuasively identifies as basic to Leninism, there are antecedents aplenty in various religious and secular traditions.

About a third of the book is concerned with Leninism itself. Besançon does not hold Lenin in much esteem as an intellectual. He finds the Bolshevik not only dogmatic but also narrowly educated. *State and Revolution* comes in for particularly severe comments (for example, "poverty of imagination," page 215). Lenin's ideas on the internal organization of the party reflect "the spirit of the *catechisms* of Bakunin and Chernyshevsky" (p. 232), two writers whom Besançon does not admire. Besançon's critique of Leninism as a system of ideas may be justified, but one finds little in his book that would enable one to understand the practical political genius that Ferro convincingly illuminates in the Lenin of the October Revolution.

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SEWERYN BIALER. *Stalin's Successors: Leadership, Stability, and Change in the Soviet Union*. New York: Cambridge University Press. 1980. Pp. v, 312. \$19.95.

This is perhaps the most sophisticated and informed Western analysis of "leadership, stability, and change in the Soviet Union" since Stalin's death. Seweryn Bialer, currently director of the Research Institute on International Change at Columbia University, has in recent years emerged as a premier interpreter of Soviet and East European developments, putting to excellent use his earlier personal experience in Eastern Europe as well as his American academic training. This book should confirm and enhance his reputation.

The first part takes the "mature Stalinist system" as a base line against which to compare the subsequent era. In the process Bialer provides a pithy inventory of characteristics of Stalinism—and of the changes that have occurred since 1953, including the transformation of leadership, the institutionalization of bureaucracy, the abolition of mass terror, the increasing role of experts, and the regime's growing responsiveness to the aspirations of major social groups. But he also stresses that the initiative for these changes has generally come from above.

Bialer also dwells, properly, on the elements inherited from Stalinism, including the conservatism and nationalism of the present regime. He aptly shows the decline in the elite's impulse to reshape Soviet society. The changes, as he sums up, have been most evident in policies; to a lesser degree in policy making; and least of all in structures.

The second part deals with the biggest legacy of Stalinism—the current elite. Here we find a comprehensive and original discussion of the characteristics of the present generation, not only of the Politburo but also of the entire elite, which will face early and massive replacement, of its demographic attributes as well as its mentalities.

Bialer then addresses the question of political stability, which he finds to have been a major trait of the Brezhnev era, and seeks to identify its major sources, such as the sense of legitimacy born of rising standards of living and increasing personal and job security; the institutionalization of politics and participation; and the expectation of social and political upward mobility. As the author makes clear in his excellent chapter on "the politics of stringency" which, he believes, lies ahead in the 1980s, the circumstances that made possible the regime's impressive albeit flawed performance and the expectations that went with it are not likely to obtain in the years ahead, given the slowing of economic growth, looming manpower and energy shortages, and a multiplication of social pathologies. In Bialer's view, it is the "national question"—the status of the non-Russian ethnic groups in the USSR—that may prove to be the most important challenge to legitimacy and stability.

A concluding part deals with foreign policy and the prospects for the future. Few specialists on modern Russia will agree with every judgment and conclusion of this stimulating volume. Even fewer can afford to ignore it: it amounts to the best effort to date to combine social science methods, a careful use of Soviet and Western sources, and personal impressions into a coherent whole. With all its unanswered questions, this is about as insightful an analysis as we can have, given the present state of the art.

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## NEAR EAST

G. H. BLAKE and R. I. LAWLESS, editors. *The Changing Middle Eastern City*. (Croom Helm Series on the Arab World.) London: Croom Helm or Barnes and Noble, Totowa, N.J. 1980. Pp. 273.

This book is more valuable for geographers than historians. Several British geographers, particularly those associated with the University of Durham's Middle Eastern program, were invited to contribute essays reviewing the current state of research on various aspects of Middle Eastern urban life. The individual contributions are quite diverse—one historical survey on the evolution of the premodern Middle Eastern city, then chapters about contemporary urban growth, migration patterns, employment, the informal sector in the urban economy, ethnic clustering, retailing patterns, urban planning, the (lamentable) state of historic urban centers, Middle Eastern small towns, and urban water problems. These studies are preceded and followed by an introduction and conclusion by the editors, G. H. Blake and R. I. Lawless, that summarize and extrapolate from the findings of the essays. Most of the chapters follow a similar pattern of summarizing what generalizations can be drawn from existing scholarship, illustrating some of the diversity of Middle Eastern urban development by examining a few case studies of individual cities, and finally suggesting problems or policies in future urban development.

The volume is a useful one. It provides an informative survey of the findings of a wide range of historical, geographical, economic, sociological, and anthropological research. Particularly valuable to the Middle Eastern specialist are the evaluations of the applicability to the Middle East of theoretical perspectives about cities developed in relation to other regions of the world. Some of the contributions present very powerful arguments for policies necessary to reverse the trends of physical deterioration and socioeconomic polarization that have characterized the development of Middle Eastern cities in the past few decades. On the whole, however, the contributors are not very optimistic about the future of Middle Eastern urban life. The final conclusion of the book's editors reflects the gloomy tone of the work as a whole; "what we must realistically expect is a continuation of contemporary trends to breaking-point" (p. 263).

The work also has its limitations. With the exception of a few new statistical computations, the essays do not make an original contribution to knowledge. In spite of the considerable range of topics addressed in the volume, there are many important issues relating to contemporary Middle

Eastern urban life that are not dealt with. This is particularly true of political questions. Other than occasional references to the range of classes and groups found in the sprawling cities of the region and the disparities of power and privilege between them, there is no systematic consideration of questions of political organizations, processes, and tensions. Even in regard to subjects that are addressed, the essays sometimes reveal the paucity of our knowledge about contemporary urban life in the Middle East. Perhaps the best example is the "informal sector" of hawkers and hustlers that has burgeoned recently: while its growing importance is noted in several of the chapters, its dynamics seem to be as yet unexplored in the scholarly literature surveyed. The most important limitation for the purposes of this review is the marginal usefulness of the volume for historians. Its one historical chapter is a survey of published research that adds nothing new to that research. The other essays contain occasional information about the historical development of the region's cities, all drawn from existing literature. Thus, while the book is a valuable review of many aspects of contemporary Middle Eastern urban development, it has little that is directly relevant to historical examination of the changing Middle Eastern city.

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ROGER SAVORY. *Iran under the Safavids*. New York: Cambridge University Press. 1980. Pp. x, 277. \$34.50.

Few books have been awaited with such anticipation by fellow Iranologists as this meticulously researched survey of the Safavid period by Roger Savory. During the late 1950s the appearance of a succession of articles on early Safavid institutions and the peculiar relationship of the Safavid state with the *silsila* of the shaykhs of Ardabil, published in the *Bulletin of the School of Oriental and African Studies*, alerted the profession to the arrival of a new young scholar in an important but also a somewhat neglected period of Iranian history. Now, more than twenty years later, we have the summation of much additional research and of judgments tempered by time. The result is a most impressive volume: a compendium of information that should take its well-deserved place on the shelves of every Middle East collection.

The Safavid period (ca. 1500–1722) was one of quite exceptional significance for the emergence of modern Iran, not least because, after centuries of alien rule by Arab, Turkish, and Mongol conquerors, the Safavids themselves came to embody a concept of national kingship that, transmitted across the po-

litical wastes of the eighteenth century, was resuscitated and enlarged first by the Qajars and then by the Pahlavis. Equally important, the early Safavid shahs so effectively imposed "Twelver" Shi'ism upon their subjects that it remains to this day the religion of a majority of Iranians. The failure of the later Safavids to maintain the "theocratic" regime of their predecessors, however, resulted in the increasing alienation of the Shi'i *ulama*, who tended to view all political authority as being illegitimate unless it conformed with what they judged to be the will of the hidden imam, a development that reached its culmination in Iran's recent Islamic revolution.

Savory is at his best when describing the rise and fall of the dynasty, tribal ambitions and rivalries, the contacts of sixteenth- and seventeenth-century Iran with the outside world, and the splendors of Isfahan in its golden age. Some aspects of the book, however, leave a less positive impression. There is a noticeable indifference to the "new" social history: for example, there is virtually no analysis of the social composition of the bureaucracy, the urban patricianate, or the *ulama*. Savory's research and writing happened to coincide, in Iran, with a period of tireless propaganda in favor of monarchy by the late shah's government. One derives from this book a sense that Savory too identifies the history of Iran with that of its rulers. It is this patina that makes his book, excellent in so many ways, seem rather old-fashioned.

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M. M. VAN BRUINESSEN. *Agha, Sheikh, and State: On the Social and Political Organization of Kurdistan*. Utrecht: University of Utrecht. 1978. Pp. xii, 466.

This is a welcome book, and it is now one of only three books dealing with the social, political, and ethnographic structure of Kurdistan that have appeared in the last thirty years. The other two are Frederick Barth's *Principles of Social Organization in Southern Kurdistan* (1939) and Basil Nikitine's *Les Kurdes, Étude sociologique et historique* (1956). There have also been some ethnographic works on certain parts of Kurdistan, the most notable being that of İsmail Beşikçi, *The Social Order of Eastern Anatolia* (1969, in Turkish). There is a paucity of information on the Kurds because significant portions of what is called "Kurdistan" in the West and by the Kurds themselves lie in the countries of Turkey, Iran, and Iraq. Most of Barth's and Nikitine's work was completed in the 1920s and 1930s respectively and on southern Kurdistan when Iraq was still ruled by the British. The British had reasons for fos-

tering Kurdish nationalism and allowed some research on the Kurds. Many of the best ethnographic accounts of the Kurds written between 1920 and 1958 were written by British political officers—Soane, Noel, Rawlinson, Edmonds, Longrigg—in Kurdistan.

The book has six chapters; the most interesting are chapters 2, 4, and 5. The central concern of chapter 2 is an inquiry into the origins and structure of tribes, the function of leadership and creation of leaders, and the economic support for the leadership role. M. M. van Bruinessen concludes that in Kurdistan all of the above processes are fluid and heterogeneous.

Chapter 4 presents a detailed analysis of the structure of the two most important brotherhoods in Kurdistan: the Naqshbandi and Qadiri orders. The author explains why these two orders, especially the Naqshbandi, have been spearheads of revolt, rebellion, and leaders in movements for independence in Kurdistan.

Chapter 5 (pp. 353–406) deals with the Shaykh Said revolt, which van Bruinessen terms "the most significant revolt in Kurdish history" because it was the impetus for the Kurdish revolts and independence movements from 1927–30 to the present. Van Bruinessen considers the Ararat rebellion of 1930 "an even more formidable threat to the Turkish government" than the Shaykh Said revolt had been. The author concludes that the revolt was a mixture of both nationalist and religious (jihad) factors. Nationalism was strongest among the originators and planners of the revolt, and their primary objective was the establishment of an independent Kurdistan. Commencing with the Shaykh Said revolt, the intermingling between the religious and the nationalist factors became less and "nationalist loyalties began . . . to lead a life of their own and have no longer such strong (religious) associations."

The weakest part of chapter 5 is the failure to establish the international pressures and policies that were confluent in the Middle East in 1925 and especially in the maelstrom that was Kurdistan. The political and international effects of the Shaykh Said revolt are worthy of note as they deeply and profoundly affected the politics of the Middle East for decades; in fact, some of the effects of the revolt are evident in the area today. For example, Great Britain did not support the rebellion, probably because the new Turkey being created under the leadership of Atatürk would have been too weakened to withstand increased Soviet pressures. A Kurdistan under British domination would have strengthened Turkish-Soviet relations. Instead the reverse happened: Turkey became oriented towards the West. This westward orientation became clear in the Turkish-Iraqi (British) treaty of June 5, 1926.

M. M. van Bruinessen has written a book every-

one interested in the Middle East will want to own (unfortunately it is already out of print). While there are criticisms that can be made regarding typesetting errors, writing, and style, nothing detracts from the value of this book. It will become a classic in its genre. Van Bruinessen is to be congratulated.

ROBERT OLSON  
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complementing such works as Gordon Waterfield's *Sultans of Aden* (1968), a detailed study of Aden's controversial first administrator, S. B. Haines, or R. J. Gavin's *Aden under British Rule, 1839-1967* (1975), a work of larger scope that remains substantially more comprehensive and readable than Kour's offering.

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Z. H. KOUR. *The History of Aden, 1839-72*. Totowa, N.J.: Frank Cass. 1981. Pp. 240. \$29.50.

In 1839, when a few hundred soldiers from Bombay seized the village of Aden, it was a sleepy hamlet of six hundred souls, rubble-choked wells, and dilapidated houses. Its past glories had dwindled, and its future was uncertain given the vagaries of British policy and the determined hostility of inland neighbors. Twenty years later, the population had reached a stable twenty thousand, trade had revived and expanded, and Aden's utility as a base had been proven in the Abyssinian campaign of 1867-68.

Zaki Hanna Kour, a Palestinian whose educational background includes Cairo, Beirut, and a doctorate from the University of London, contributes in this short book principally to the administrative history of Aden from the moment of British acquisition to the early 1870s, when circumstances were altered by the Ottoman occupation of Yemen. He has dutifully mined the archives in the India Office Library, notably the Bombay Proceedings and the Aden Residence Records, focusing upon the settlement's physical growth, trade, administration, and, in the last half of the work, relations with the sultans and tribes of Lahej. Insofar as the book has a theme, it is the interesting conflict between civilian and military authorities, though the confusion of aims—should Aden be a military base or commercial entrepôt?—has been clear for some time. Much factual detail, not readily available elsewhere, is presented in support, but it is not always selected with discrimination. Equal weight is given to the unequal subjects of trade statistics, the flow of letters through the post office, the principal problems faced by health authorities (scurvy and ulcers), and the functions of the harbor police (a corporal and seven men). The book's most glaring weakness, however, concerns the treatment of the town's physical evolution. Only a rudimentary and nearly illegible chart from 1839 is offered in illustration, and without a decent map these sections are intelligible only to the expert or the reader who equips himself with additional sources.

Nevertheless, within its limits, the book does fill out some of the corners for specialists on the area,

## AFRICA

KENNETH J. PERKINS. *Quids, Captains, and Colons: French Military Administration in the Colonial Maghrib, 1844-1934*. New York: Africana. 1981. Pp. 278. \$24.50.

Kenneth J. Perkins has made a signal contribution to our knowledge by publishing the first full-length study of the military administrators in the French Maghrib. In each of the French territories by an odd coincidence the influence of military rule lasted a couple of decades—in Algeria 1844-71, in Tunisia 1887-1914, and in Morocco 1912-34. Called *bureaux arabes* in Algeria, *service des renseignements* in Tunisia, and *service des affaires indigènes* in Morocco, the services administered "tribes" considered too turbulent for civilian rule.

Well informed by a comparative approach and looking at similar colonial bureaucracies in the British empire and among civilian corps in the French empire, Perkins provides a fine introduction on the training and recruitment of the officials. France's military officers were graduates of military academies and had the secondary education that in the nineteenth century was available for the middle classes. They came from a higher stratum than most of the French civilian administrators overseas. They also often had previous overseas experience, although they tended to lack competency in Arabic—this in spite of special efforts to provide training for the officers.

Faced with existing authorities, the administrators used them to extend French control and on the whole did little to destroy them. They did make sure, however, that they were instruments of French policy; those who resisted were replaced without hesitancy.

The study provides a general sketch of the efforts the military administrators made to extend cultivation and to build roads, bridges, schools, and hospitals. Given the rich documentation that Perkins has worked with, one misses a clearer description of the long-term vision these officials had of what the outcome of French policies would be.

Perkins sees clear differences between the three administrations. The Algerian one had to cope with massive resistance from the settlers who were eager



to deprive the Arabs of the land and who resisted the type of paternalistic protection that the administration provided the Arabs against further European encroachment. In the other two Maghrib countries the settlers were never as numerous. Also a number of lessons had been learned from Algeria; the administrations in Tunisia and Morocco were more enlightened. Some of the values of the officials had also changed. By the turn of the century the humanity of the indigenous peoples had been given greater attention than was the case half a century earlier.

Although the comparisons of the three services are instructive and the author reminds us that we have to remember the changing milieus in which they functioned, we need a more developed sketch of these environments. Not only the evolving nature of the indigenous populations but also the French administrative structure and the leading personalities in the colonial capitals and Paris could use greater emphasis. Although French administration in Algeria until 1871 can generally be understood in terms of the functioning of the *bureaux arabes*, from very early on in Tunisia and Morocco the civilian corps were far more important in shaping these areas. Maybe Perkins will provide us with a study of these civilian corps in the future.

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CLAIRE HIRSHFIELD. *The Diplomacy of Partition: Britain, France, and the Creation of Nigeria, 1890-1898*. (Studies in Contemporary History, number 2.) Boston: Martinus Nijhoff. 1979. Pp. 234. \$27.00.

PHILIP AIGBONA IGBAFE. *Benin under British Administration: The Impact of Colonial Rule on an African Kingdom, 1897-1938*. (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press. 1979. Pp. xv, 432. \$35.00.

So different are these two books that it is momentarily hard to believe that they address the same generic historical circumstances, in neighboring places, in contiguous periods. If they did not know that the two works were published in the same year, students of African history would conclude that they demonstrate the changes of the last two decades in writing about that continent and its late nineteenth-century encounters with Europe. Philip Aigbona Igbafe's book is a fine example of the current genre; Claire Hirshfield's is an anomaly these days.

Not that every historical book set in Africa need be about Africa; Hirshfield's is rather, as its title says, a diplomatic history of what the French and

the British were doing to each other on a bit of Africa's turf. The author has carefully made her way through Foreign Office and Colonial Office series for the years 1890-98, their counterparts in Paris, the private papers of some of the British actors, memoirs, parliamentary debates, and newspapers of the day. Her bibliography and footnotes show use of substantial secondary material too, some of it recent and by Nigerian and non-Nigerian authors who show the value of including the African dimension, largely omitted in her book.

A very brief introduction states that "the partitioning document" was to have great impact on the lives of millions of Africans. It also contains a basic confusion in terminology: the Niger Bend is usually understood to be in present-day Mali, not to be the change in the river's direction in northwestern Nigeria, which is what she is in fact describing.

Four chapters follow: "The Genesis of Interest," "The Developing Crisis," "The Deepening of the Crisis," and "The Final Stage: Climax and Settlement." A conclusion states mainly that "for neither France, nor in the final analysis Britain, was the disposition of the Niger Bend [*sic*] or the Sokoto Empire vital." This is not news, nor is it news that Sokoto felt more threatened by the forces of Rabeh in Borno than by "the minor menace" the Europeans seemed by comparison—even though, finally, it was the British who would bring the formal demise of the Sokoto caliphate. There the book ends, a competent treatment of a few actors in a cast of thousands, but actors largely known about already.

Igbafe's book presents a marked contrast. In the 1960s doctoral students in history from the University of Ibadan began examining how in fact—in contrast to British administrative theory or colonial perception—Nigerians from different parts of the southern, non-Islamic part of the country first came to terms with the occupying presence. Their research looked at the resulting institutional innovations, adjustments, and adaptations made through both misunderstanding and understanding of traditional political and administrative arrangements. Such studies have told students of Nigeria's past a great deal. The authors, trained historians familiar with the contexts and languages of their subjects, have illuminated the period from resistance and conquest at the end of the nineteenth century to the 1930s, by which time administrative, educational, and other related patterns of adaptation were more or less sorted out.

Igbafe's work is another such effort, and a particularly impressive and stimulating one. He used archival sources, Foreign and Colonial Office series, and other documents in London but also the dispatches, kept in the Nigerian archives, between local British officials and London; intelligence reports; government publications; and newspapers. A



range of oral evidence acquired over many months skillfully supplements, checks, and adjusts written sources.

The study begins with the indigenous background and the fall of Benin in 1897, the latter an episode painted often at the time and since in superficial strokes and lurid detail and here handled in a balanced, comprehensible way. Igbofe is indeed impressive in his balance; he shifts his point of view between indigenous and British perspectives, sensibilities, and perceptions to good effect. He moves from the consolidation of British rule, for example, to "the interregnum, 1897-1914," referring to Benin's traditional kingship. From there he takes up "Restoration, conflicts and institutional adaptations," showing how adaptation to this effort to make indirect rule work took place, in the kind of detail that is essential to knowing which of the often-made generalizations on the subject bear weight. Not surprisingly, he reveals a more complicated process than historians often thought applied.

Two chapters follow that explore what kinds of judicial changes took place and show the fundamental impact those changes had on society. Igbofe likewise deals with changes from tribute to direct taxation and in a particularly valuable chapter treats "social conflicts and the administrative and judicial reorganization" of the 1930s, illustrating the dynamic side of continual British attempts to make a system they considered acceptable that was also—despite straining the cultural and historical sinews—workable.

He then turns in the penultimate chapter to "Aspects of British Economic Policies in Benin," which seems not quite integrated into the rest of his approach, and then concludes with "Developments after 1938," in which the author states in the final sentence that "one fact is clear: the survival of much that was Benin in pre-colonial times bears today a most powerful testimony to the resilience of African kingdoms, their traditions and practices" (p. 390). It is an important point, and one that neither Nigerians nor observers interested in Nigeria's present should overlook, for Benin and elsewhere. Igbofe has shown the basis for the statement fully and clearly; he has tried to incorporate all points of view. If he fails occasionally to put aside twentieth-century Nigerian reactions to British lack of understanding of Africans in earlier eras, that is the more understandable when the Hirshfield book demonstrates how Westerns can still take so Europe-centered an approach. To dissect African dimensions is more difficult, and it necessitates the introduction of many proper nouns, not to mention institutions, unfamiliar to outsiders, both non-African and African. Igbofe has faced those difficulties and written a

book that is readable, although obviously some background will increase a reader's comprehension of the important points he makes.

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DAVID E. SKINNER. *Thomas George Lawson: African Historian and Administrator in Sierra Leone*. (Hoover Colonial Studies.) Stanford: Hoover Institution Press. 1980. Pp. xiv, 240. \$10.95.

This book by David E. Skinner is the sixth title in the "Hoover Colonial Studies," an important series to anyone interested in modern imperialism. Unlike the others in this series, it is not just a narrative, and this is its greatest weakness. More than half the work is devoted to selected correspondence written by Thomas George Lawson from 1875 to 1886 abstracted from the eleven volumes of his reports located in the Sierra Leone archives in Freetown. Those published in the book are singly and collectively interesting to scholars who specialize in Sierra Leone but are less so for those who do not. One wonders why Skinner did not use the mass of material available to him to expand upon Lawson's long and distinguished service to the crown. Fewer than fifty pages are devoted to the narrative of Lawson's life and activities, and this section is by far the most interesting and valuable part of the book.

Lawson, however unique in the Sierra Leone service, represents very well the African evangelical, Western-oriented, transitional society of nineteenth-century Freetown. He was the second son of George Acquatay Lawson, either a Fante or a native of the Little Popo region, who by his skills had made himself king of that area. He had intended his son to be educated in England, but in 1825, at the age of eleven, young Thomas stopped in Freetown and became the ward of a prosperous timber merchant who secured for him an education in local schools. From this time until his death Thomas was a valued citizen of Freetown and after 1860 a loyal subject of the crown. By 1846, because of his knowledge of languages, he was employed by Governor Macdonald to act as an emissary to interior tribes. In 1852 Lawson became the government messenger and interpreter, a position he developed to the point where he was "essentially a director of foreign affairs." Soon after becoming a British citizen he offered to help secure his family's territory at Little Popo for Great Britain. Lawson was admired by the many governors under whom he served, and he carried out a number of important trade and political missions into the hinterland of Freetown. In 1878 he was placed in charge of caring for distinguished visitors, another important position. Lawson retired in

1888 after a forty-two year career as a servant of the British administration.

Such a brief sketch of Lawson's long career indicates why we need to know more about his personal life; his interaction with his superiors and business associates; his contacts with the Christian African society of Freetown, the rulers of the interior regions, and his family at Little Popo. Skinner's work whets, but does not satisfy, one's appetite for more information about this fascinating man and the composite African society that he represented.

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THOMAS J. LEWIN. *Asante before the British: The Prempean Years, 1875-1900*. Lawrence: Regents Press of Kansas. 1978. Pp. xii, 312. \$15.50.

The Asante (Ashanti) empire, one of the most complex and important states in precolonial Africa, continues to attract scholarly attention. Thanks to the relative abundance of data and the studies of Ivor Wilks and other able historians, we probably know more about Asante, at least in the nineteenth century, than about any other African state. This book, based on a dissertation supervised by Wilks, is a solid contribution to Asante studies and to the whole thriving field of African history.

Thomas J. Lewin describes and analyzes developments in the decisive quarter-century between the British sack of Kumase in 1874 and the final colonial conquest in 1900. Military defeat, new economic forces, chronic disputes between the central government and local authorities, and the continuing British threat set the stage for prolonged political confusion and serious disorders in Asante. Lewin gives a clear but necessarily complex picture of intrigue, maneuver, ambition, and statesmanship. The focus on "hawk-dove," "centralist-localist," and "free trade-state monopoly" viewpoints lends coherence to a generally convincing and well-written account. In contrast to the British view, Lewin argues that the election of Prempe as Asantehene in 1888 marked the beginning of a real restoration of Asante's fortunes. There is much truth to this argument, although the author may be a bit over-optimistic about the progress made and perhaps gives Prempe too much personal credit. The new Asantehene was able and honorable, but he was, after all, only twenty at his formal installation in 1894. The last chapter is dramatic and well done, though perhaps a bit too short. The British takeover in 1896 and the exile of Prempe are sudden and stunning, even with the advantage of hindsight. Oral testimony gives a vivid picture of the astonishment and dismay of the Asante over this piece of imperial treachery.

This book is based on extensive research in published and archival sources and, very importantly, on dozens of interviews conducted in 1970-71 with elderly Asante and descendants of important figures. A number of these informants gave remarkable and now irreplaceable eyewitness accounts, which Lewin uses with good effect and, I think, sufficient caution, to supplement the writings of outsiders and create a "feel" for the times. Specialists will want to consult the full texts of the interviews in volume 2 of the author's dissertation (Northwestern, 1974).

This is a good and important study, written from an Asante rather than a British perspective. A glossary, several appendixes, a substantial index, and numerous photographs and maps add to the value and utility of the book.

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AUGUST H. NIMTZ, JR. *Islam and Politics in East Africa: The Sufi Order in Tanzania*. Minneapolis: University of Minnesota Press. 1980. Pp. xvi, 234. \$20.00.

August H. Nimitz, Jr.'s book, a revised and shortened version of his 1973 Indiana University political science thesis, sets out to analyze relations between Islam and politics in East Africa, with specific attention given to the role of Sufi brotherhoods (*tarīqas*). Nimitz asserts the aim of the study to be an explanation of behavior, not a mere description of data, and he attempts to secure a working hypothesis of value beyond East Africa, closing with thirteen general propositions drawn from the research. That attempt, drawing upon Ralf Dahrendorf's theoretical perspectives on conflict groups and social change and Robert Salisbury's microanalytic models of interest groups, is probably of greatest interest to similarly inclined political scientists, since many of the propositions appear to be little more than common-sense consequences of the data presented. Nonetheless, whatever the reaction to the general approach, Nimitz has made a major contribution to the understanding of the East African past, utilizing exceptional—for scholars studying East Africa—linguistic skills in Swahili, Arabic, and German. The only notable gap in the research is the failure to consult the Paris archives of the important Holy Ghost Mission or the impressive 1966 University of London thesis by John Kieran drawn from these and other relevant materials.

Within a satisfactory historical introduction, which discusses the development of an East African Sunni community, Nimitz illuminates the role of *tarīqas*, institutions hitherto little understood in East

Africa. B. G. Martin's earlier work, culminating with *Muslim Brotherhoods in Nineteenth-Century Africa* (1976), whetted appetites, but Martin's mastery of Arabic materials was not matched by knowledge of other relevant sources. Nimtz passes well beyond Martin's speculations, convincingly attributing the steady growth of Islam in East Africa after World War I to the work of *tariqas*. The essential core of Nimtz's scholarship is the data gained, from archival and oral sources, in the present-day Tanzanian town of Bagamoyo, the principal ivory-exporting port of the East African littoral until the German conquerors developed nearby Dar es Salaam. Through the German and British colonial periods and in the years after Tanzanian independence (1961) Nimtz portrays the tensions among Bagamoyo's various ethnic subdivisions, relating the growth of *tariqas* to persistent friction between the Arab and African communities. Nimtz concludes, on convincing evidence, that most Muslims in East Africa were converted through *tariqas*, organizations that for the first time allowed Africans to rise to positions of leadership in the Muslim community. Biographical studies of selected individuals, men such as Shaykh Ramiya, originally a slave from Man'yema who became a prominent Bagamoyo religious figure, and his son, Muhamad ibn Ramiya, a modern religious and political leader, add considerably to the strength of Nimtz's arguments. *Islam and Politics in East Africa* should serve as a significant stimulant for future scholars who seek to comprehend the Islamic aspects of nineteenth- and twentieth-century East Africa.

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#### ASIA AND THE EAST

CHÜN-FANG YÜ. *The Renewal of Buddhism in China: Chu-hung and the Late Ming Synthesis*. (Buddhist Studies and Translations or IASWR Series.) New York: Columbia University Press. 1981. Pp. xvi, 353. \$25.00.

One of four eminent monks identified with the late Ming revival of Buddhism, Chu-hung (1535–1615) started life as Shen Fo-hui, the son of a Chekiang literatus. The young Shen studied for the civil service examinations even as he became a lay believer in Buddhism. The seriousness of his commitment to a career in government is evidenced by his vow to become a monk if he did not succeed in the provincial examinations by the age of thirty and in the examinations at the capital by the age of forty. Of course he failed, and by the time he was thirty-one he had witnessed the deaths of his son, father, wife, and mother. It is telling that it was with the encouragement of his second wife, who eventually became

a nun, that he was able in 1566 to carry out his intention to have his head shaved over the objections of his remaining family and friends. Having spent a few years in travel and study, the new monk, Chu-hung, settled in about 1571 at Mount Yün-ch'i in Chekiang near Hangchow. After he unwillingly demonstrated his magical powers by relieving a local drought and ridding the neighborhood of tigers, the abandoned temple complex there was rebuilt for him by grateful villagers.

Chu-hung served the rest of his life as abbot at the Yün-ch'i monastery, which he sought to make into a model of Buddhist practice in order to reinvigorate what was widely perceived as a secularized, degenerate monastic system. He emphasized moral conduct and the invocation of the Buddha's name (*nien-fō*) to the neglect of theological speculation, Ch'an meditation, and clever talk about "public cases" (*kung-an*, *kōan*). He used Pure Land teachings and even Confucian ethics to restore discipline to the religious endeavors of the Ch'an-dominated monasteries. In her sixth, seventh, and eighth chapters Chün-fang Yü discusses the political and religious reasons for the declined condition of the monastic order in late Ming and analyzes Chu-hung's syncretic efforts at reform.

In the third through fifth chapters, Yü treats Chu-hung's attempts to reach out to the lay world. Again the themes are syncretism, combining Buddhism with Confucian moral teachings as well as Pure Land with Ch'an; the call for self-sacrificing religious activity, exemplified in the implementation of the ideal of "non-killing" through the organization of "societies for releasing life"; and moral rules sanctioned by a system of quantified merits and demerits. In 1604 Chu-hung contributed to the mainly Confucian-Taoist tradition of "morality books" (*shan-shu*) by writing a *Record of Self-Knowledge* (*Tzu-chih lu*). A reliable translation of the *Record*, which introduces Buddhist precepts as an important component, is included as appendix 1.

Yü lucidly summarizes the Buddhist and political contexts as they bear on Chu-hung's career. The book thus serves as an excellent introduction to the place of Buddhism in the late Ming religious scene. It also can be read on another plane. Chu-hung's critiques and his attempts to reform the two societies in which he participated—lay and clerical—have parallels in some of his literati contemporaries' efforts in theirs. Chu-hung is a striking example of the late Ming concern with reimposing, even reinventing, a discipline that would bring order to a disintegrated world.

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JOHANNA MENZEL MESKILL. *A Chinese Pioneer Family: The Lins of Wu-feng, Taiwan, 1729–1895*. Princeton:

Princeton University Press. 1979. Pp. xi, 375. \$20.00.

Johanna Menzel Meskill's work is a study of a Chinese pioneer family, the Lins, in eighteenth- and nineteenth-century Taiwan. It is thoroughly researched, well written, and excellently argued. It will be useful to students of comparative studies of colonization as well as to those interested in Ch'ing China.

In addition to telling an interesting story about the Lins' transformation (during the course of one and one-half centuries) from frontiersmen to local strongmen and finally to gentlemen, the author deals with the larger topic of Chinese expansion and integration of the newly colonized island with the established regions of the empire. First, Meskill discusses the island's "culture of violence" (p. 87) and traces its origin to "the endemic state of violence as local groups armed themselves for self-defense and spoliation" (p. 254) of southeastern China wherefrom the settlers originated. The frontier situation of Taiwan, where conflicts over land rights were exceedingly numerous and government control was ineffective, further helped to militarize the island society.

Most of the steps that made Chinese colonization of the new territory possible were achieved with little or no aid from the government and often in spite of official prohibitions. In fact, the local governments performed few services and imposed minimal burdens. "Even such basic tasks as the administration of justice and the maintenance of internal order rested largely with private groups who settled such matters as best they could—through community pacts, blood revenge, and armed conflict" (p. 256). "In this atmosphere of increasing strife, communal and clan groups rallied around vigorous leaders. Assertive and skilled in combat, such men organized groups of fighters for more effective action, whether in self-defense or for aggrandizement. Known as local strongmen (*t'u-hao*) these leaders benefited from the chaos and in turn perpetuated it" (p. 88). Meskill specifically emphasizes the role played by these men of private armed power in running local affairs. Citing Philip Kuhn's *Rebellion and Its Enemies in Late Imperial China*, which brings forth the fact that mid-nineteenth-century local rebels arose from among the leaders of initially apolitical armed groups in the established mainland provinces of China, she justifiably questions the validity of "the dominant historiography that has painted a misleading picture, one of a profoundly pacified country in which men of distinctly civilian ethos ruled by cultural guidance over a population that purportedly loathed violence, where the man of the truth was always mightier than the man of the sword" (p. 262).

Some strongmen led rebellions, other fought reb-

els on the government side. At different times the Lins had been on one and then the other side of the law. In the 1880s, for the sake of survival, the new leaders of the family finally and wisely decided to leave the strongman tradition behind to make the transition to "gentlemen." Meskill insists that the reason the Lins made such a decision was because the family had changed but the island's general circumstances had not—"conflicts, private violence, and the power of strongmen continued to the end of the century" (p. 179). Some historians would have reservations about Meskill's observation. In his study of Ch'ing Taiwan, Wen-hsiung Hsu, for example, concludes that "after the mid-nineteenth century, with the frontier disappearing, popular uprisings became less common and communal strife did not occur . . . on the west coast of Taiwan" ("Frontier Social Organization and Social Disorder in Ch'ing Taiwan," in Ronald G. Knapp, editor, *China's Island Frontier* [1980], p. 105). Such being the case, it can be argued that the Lins and probably many other local strongmen were ready to and did make the necessary adjustment to become more respectable, in the eyes of the government officials, as the new circumstances in the second half of the nineteenth century dictated.

This book contains a good number of typographical and romanization errors and at least one factual error—the inaccurate assertion that the Chinese use of machine guns in Taiwan came as early as 1886 (p. 188). Such minor flaws, however, do not diminish the value of the book, which is a major addition to the expanding field of Ch'ing China studies.

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PIERRE-ÉTIENNE WILL. *Bureaucratie et famine en Chine au 18<sup>e</sup> siècle*. (Civilisations et Sociétés, number 66.) New York: Mouton Éditeur or École des Hautes Études en Sciences Sociales, Paris. 1980. Pp. 312.

Premodern China rarely enjoyed a year without serious drought or flood. A county or sometimes a whole province would then be visited by the familiar crisis. Peasants trekked to the pawn brokers, mortgaged their land, filled their stomachs with chaff or "sweet earth," sold their daughters. There could be jacqueries, banditry, executions, and refugees. Amid this disruption, the state manifested its vigor—or its impotence.

Pierre-Étienne Will has focused on the view from above, on bureaucratic efforts to mitigate the effects of famine. He generalizes the experience of the Ch'ing dynasty's eighteenth-century heyday against the wider background of the period ca. 1600–1850. The monograph rests heavily on a genre of famine



administration literature, particularly a work called the *Record of Assistance (Zhenji)*. This extensive collection of documents describes the relief program organized by the book's compiler in 1743-44. Supplemented by similar materials dating from the seventeenth to the nineteenth centuries, this source enables Will to reconstruct the typical pattern of bureaucratic response to famine.

He shows that such disasters presented China's rulers with an enormous administrative challenge. The official had to know which places were affected and to what extent. He had to identify the specific aid recipients. He had to monitor the sub-bureaucrats, who were both his agents in implementing the program and a serious source of abuse. He also had to decide whether aid would take the form of money or grain and whether to offer loans, sale of foodstuffs at reduced prices, or outright charity. Peasants must be induced to stay put, while the displaced must be assisted to return, but in such a way as not to attract more peasants to the administrative centers. And for a program to have any success, the state must possess adequate financial surpluses, grain reserves, and administrative integrity.

One is not surprised that famine relief was an imperfect thing at best. In times of dynastic disarray, such as preceded and followed the eighteenth century, the state's response did not much transcend words and gestures. The real burden shifted to the local gentry, who could not fully shoulder it. But Will demonstrates that during a century or so of dynastic vigor the means, the procedures, and the will were present and were in fact mobilized when occasion demanded.

The author relates famine administration to other historical issues as well: demographic changes, spatial patterns of economic development, shifting social and political balances, and dynastic decline. He makes no claim that this monograph exhausts the subject. But it is an excellent beginning. It also adds important weight to recent scholarship showing that the premodern Chinese bureaucracy could be, within its technological limitations, responsive and effective.

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PARKS M. COBLE, JR. *The Shanghai Capitalists and the Nationalist Government, 1927-1937*. (Harvard East Asian Monographs, number 94.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1980. Pp. xiv, 357. \$15.00.

It is always tempting for a social scientist to "psychoanalyze" a political party or a government. Why does a government have a certain ideology or adopt a certain policy? Is it controlled by certain social

classes or certain vested interest groups? The author of this informative book, Parks M. Coble, Jr., attempts to examine whether the Nationalist party (Kuomintang) under Chiang Kai-shek was controlled by or represented the interest of the Chinese capitalists, especially the Shanghai capitalists, during the period from 1927 to 1937.

The author rejects the thesis that the Nationalist government represented the interests of the capitalists or that the latter were able to exercise political control over the government. Instead, he concludes that Chiang "freely ignored the collective opinion of the Shanghai capitalists" (p. 262) and that the capitalists were "denied a political role in the Nanjing Government." In the author's view, the Kuomintang did not systematically represent any social class; it was essentially an autonomous force based on military power.

The author reaches his conclusion by examining a series of historical events. He describes how the Shanghai capitalists lost their political control of Shanghai when the Kuomintang first arrived in that city in 1927-28 and applied coercive methods in selling them government bonds; how T. V. Soong failed in persuading Chiang Kai-shek to abandon anticapitalist policies; how Soong and the Shanghai capitalists failed to curb Chiang's policy of military and, hence, deficit spending; how H. H. Kung engineered a coup in 1935 by which the government gained control of the Bank of China and Bank of Communications, thus further diminishing the political influence of the business group; and how the introduction of the *fa-pi* eroded the Shanghai financial capitalists' remaining strength.

Coble's main thesis that the Nationalist government was free from the Shanghai capitalists' control is probably correct. After all, Sun Yat-sen's Principle of People's Livelihood (*Min sheng chu-yi*), which was the guiding economic ideology of the Nationalist government, never granted the capitalists a free hand to pursue their interests. On the contrary, it specifically advocated limitation of private capital.

But Coble is much too loose in formulating his hypothesis. When he states that the Kuomintang did not represent the interest of, or was not controlled by, the capitalists or any social class, what exactly do the words "represent" and "controlled" mean? Unless they are defined in testable or verifiable terms, the author's thesis cannot be rigorously substantiated or rejected. I should also think that the question at hand would have been understood in greater depth if Coble had analysed the incidence of the burden of the tax system and the rates of return on investments by the capitalists. The true test as to whether the capitalists were hurt would seem to be whether they prospered. Perhaps the author did not wish to examine the issue by technical economic analysis. On the whole, the book is well written, asks an important question,



and provides a stimulating starting point on the nature of the Nationalist government before 1937.

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JOHN Z. BOWERS. *When the Twain Meet: The Rise of Western Medicine in Japan*. (Henry E. Sigerist Supplements to the Bulletin of the History of Medicine, new series, number 5.) Baltimore: Johns Hopkins University Press. 1980. Pp. xi, 173. \$14.00.

John Z. Bowers's new book on Western physicians in late nineteenth-century Japan has several notable strengths. It is based on new research. The material is inherently engaging. And it contains useful—and scarce—information. One sees all these qualities in the chapter on Otto Mohnike (1814–87), who introduced the stethoscope to the Japanese in the 1850s. Bowers introduces Mohnike's perceptive remark to the effect that the Japanese, under proper leadership, would likely make "greater progress in . . . medicine than in the other arts and sciences" (p. 13). He describes, through Mohnike, the manufacture of Japanese dentures, which were "to be preferred over those made in Europe" (p. 17). And he compares the Japanese use of moxibustion as a "springtime prophylactic" with the European practice of bloodletting "done in the Spring" (p. 13). Nor is the Mohnike chapter an isolated example. Other sections are also based on the subjects' original writings and contain pungent comments on Japanese students and colleagues and on working conditions in Japan.

Bowers's strength, however, is also his greatest weakness. His book has no thesis. It asks no particular questions. And it frequently confuses historical fact with the Western doctors' opinions. He says, for example, that the "study habits of Japanese students boasted none of the methodical steadfastness and punctuality characteristic of the Germans" (p. 76), leaving the reader uncertain whether this is his opinion or simply that of a historical figure. He cites Benjamin Mueller's contention (p. 74) that medical students should have a "strong physical condition" because, in Bowers's words, "The practice of medicine [in Japan] has historically been a haven for the physically and mentally unfit." Bowers himself may not believe this, yet he quotes it and similar statements as though they were fact.

The book is also improperly titled. Readers of earlier works by Bowers will recognize *When the Twain Meet* as a sequel to his *Western Medical Pioneers in Feudal Japan* in that both describe the careers of European physicians who worked in Japan during the Tokugawa and Meiji eras respectively. This perception is strengthened by the discovery that fundamental issues pertinent to "The Rise of Western Medicine in Japan" (the subtitle) are not and can-

not be adequately treated by the book's almost exclusively Western focus.

There are also factual errors. Emile Roux did not share a Nobel Prize with Von Behring or anyone else (pp. 111–12). The first Christian infant was born in Japan during the sixteenth century, not 1860 (p. 49). The bacteriologist Kitasato Shibasaburo was born in 1852, not 1856 (p. 110). And *Ko-seisho*, not *Kaiseijo*, is the term for Japan's present-day Health Ministry (p. 110). Moreover, the bibliography shows signs of editorial carelessness. Two Japanese writers are cited by their given names (Uchiyama under "Koichi" and Sato under "Hachiro"), while the Dutch physician Pompe van Meerdervoort is listed under the "M's."

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JOHN CURTIS PERRY. *Beneath the Eagle's Wings: Americans in Occupied Japan*. New York: Dodd, Mead. 1980. Pp. xvi, 253. \$12.95.

This book, written in brief compass, captures the essence of the American experience in occupied Japan. John Curtis Perry states that it is not written for scholars, but in fact it is for those scholars who would appreciate *sumi-e*, the Japanese paintings that portray a scene or suggest a world of feeling with a few skillful brush strokes. This important episode in America's history is related with perception and literary grace.

Perry sees the occupation as a great success, an assessment that few would argue with today. The Americans came into Japan full of their own vitality and energy, convinced of the superiority of their culture and its suitability for Japan, and unencumbered with much knowledge of Japan's history and culture. MacArthur epitomized the Americans in Japan with his enormous self-confidence, his ethnocentricity, and his ignorance of Japan's heritage. These American characteristics might have been reasons for failure, yet paradoxically the occupation was an extraordinary success, "a landmark," writes Perry, "in human history" (p. 215).

However little Americans knew about the Japanese, Perry notes that occupation policy actually did not clash head-on with Japanese ways of doing things. The nation was ruled through the Japanese government, making local military government units largely superfluous. The technique most widely used by occupation officials was hortatory: advice, counsel, and visits by experts invited to Japan by the supreme commander. This worked because of the extreme deference shown to the occupiers by the Japanese people and their leaders. Occupation policy likewise did not result in the destruction of Japan's culture. Old Japan hand Eugene Dooman questioned whether Americans had

the right to reorder the social structure of Japan and feared that Japan's cultural integrity would be destroyed. Yet, although the occupation did remake the social, political, and economic structures of Japan, Dooman's fears proved to be overdrawn. Just as Japan's culture had shown a great degree of resilience in its encounter with the West in the nineteenth century, it displayed a similar resilience even while the nation was being reformed by the exuberant Americans.

The biggest miscalculation of the occupation, it turns out, was the failure to grasp Japan's future. Perry writes that an economist in 1947 calculated prospects for Japan's iron and steel industry as "not very promising" (p. 123). As the occupation was nearing its end, another expert expressed doubt that Japan could earn enough from foreign trade to regain the moderate standard of living that had prevailed before the war!

In future years scholars doubtless will be writing many specialized studies relating to the occupation of Japan, for the historical materials are voluminous and the subject is of great significance. Meanwhile we are much in debt to Perry for this fine interpretive portrait of the American experience in occupied Japan.

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RICHARD B. BARNETT. *North India between Empires: Awadh, the Mughals, and the British, 1720-1801*. (Center for South and Southeast Asia Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1980. Pp. xvii, 276. \$25.00.

The title of this work is misleading, since Richard B. Barnett presents the reader with much more than a descriptive regional history of the province of Awadh between the respective dominance of the Mughals and the British. The author portrays Awadh as a case study to advance our knowledge of the process of iraperial decline, emergent regionalism in the face of this decline, and the struggle to retain regional autonomy against a successor imperial power. Central to this process, Barnett asserts, is an exchange of political resources among the various actors whose aspirations, expectations, and exactions determined the ebb and flow of the imperial tide.

In Awadh the initial steps toward autonomy were taken by Saadat Khan, the subahdar or governor of the province who was appointed to this post by the Mughal emperor Muhammed Shah around 1720. By the skillful and timely use of Mughal authority, Saadat Khan secured his power base in Awadh, and, with Mughal sanction, he appointed his son-in-law deputy governor. The intrigue and politics of

the Mughal court and the turmoil caused by the invasion of Nadir Shah permitted Safdar Jang, Saadat Khan's son-in-law and successor, to extend further the degree of autonomy exercised by the governor of Awadh.

With the eclipse of Mughal power, the real threat to Awadh's autonomy came from the expansive English power emanating from Bengal. The battle of Baksar, which the author describes as "perhaps the most important battle the British ever fought in South Asia" (p. 64), settled the issue and laid the foundation for the subsidiary relationship that was to exist between Awadh and the British for the remainder of the century.

The transition from functional sovereignty under the Mughals to dependency under the British was a painful process for the rulers of Awadh, but the astute management of their resources and the resiliency of the regime's structure vis-à-vis the acquisitive designs of the East India Company enabled the *nawabi* to survive in the eighteenth century. By effectively dispersing resources to the local level through the process of *ijarahdari*, the revenue system whereby a fixed amount was paid to the regime and the remainder held by the collector-lease holder, and by sequestering funds to noble dowagers whose resources remained incomprehensible to the British, the *nawabi* maintained a surprising degree of autonomy through the eighteenth century. By this fragmentary system of resource diffusion, the political structure in Awadh with its overlap of caste, kinship, and local networks remained largely intact and impenetrable by the English through the turn of the century. Baffled by their inability to understand or to control the manipulation and management of resources within Awadh, the English accepted, during the eighteenth century at least, the troublesome resiliency of a regime that they could neither discern nor dominate.

In his account of the history of Awadh and in his analysis of the relationship among the province, the Mughals, and the English, Barnett makes a major contribution to our knowledge of South Asian history and of the imperial process itself.

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EDWARD INGRAM. *The Beginning of the Great Game in Asia, 1828-1834*. New York: Clarendon Press of Oxford University Press. 1979. Pp. xi, 361. \$29.00.

Throughout much of the nineteenth century, British policy formulated for the security of British India, as well as that formulated to maintain England's European interests, which Edward Ingram calls the European balance of power, was usually uncoordinated. The Foreign Office and the Cabinet

demonstrated time and again their disinterest in Indian affairs; the exceptions occurred only under the influence of moral outrage or the intention to spend money. Humbug and parsimony aside, India was a relatively quiet backwater for the Foreign Office in the first third of the nineteenth century. This left the formulation of Indian policy to the Board of Control in London and, at times, to a dominant governor-general in India. The author, however, believes that there existed a close connection between the European balance of power and the security of British India, and he excoriates Castlereagh, Canning, and Palmerston, as well as historians such as Webster and Temperley, for ignoring India when discussing Europe. It is far from clear to this reader, despite Ingram's assertions, how British India was so significant for London down to 1830. After 1870 India's importance is much clearer.

But the book, as the title indicates, is less about the aforementioned lack of policy coordination than about the origins of the need to defend British India from the impact of Russian expansion and influence in Iran, Turkestan, and Afghanistan—the "Great Game." Ingram gives the "credit" for devising a successful strategy to contain or deflect Russian expansion to two men—Edward Ellenborough, president of the Board of Control, and Henry Ellis, holder of various posts in India and London. This solution was to turn the Ottoman empire and Iran into buffer states where Britain as well as Russia would keep out. This, Ingram argues, was far preferable to an attempt to establish a British protectorate because Russia's geographical proximity to central Asia would always leave it with the advantage.

The book is based on India Office records in the India Office Library, Foreign Office letters in the Public Record Office, and relevant private papers in those two archives and the British Library. Printed sources are not neglected, but some of the author's interpretations may have been altered had he been able to make use of the recent book by M. E. Yapp, *Strategies of British India*, which covers some of the same ground.

This work will be of interest to those who follow the Eastern Question, especially the crisis of 1832–33; Anglo-Iranian relations between 1801 and 1834; and, of course, the "Great Game," devising the best way to secure British dominion within India. There are five good maps and a useful index. The writing style is lively, epigrammatic, and reminiscent of that of A. J. P. Taylor, another historian who managed to concentrate upon Europe while leaving India in the background. But, if the manner of writing is sometimes provocative and exaggerated, it is also, and equally, pungent, interesting, and spirited.

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MANILAL BOSE. *British Policy in the North-East Frontier Agency*. New Delhi: Concept Publishing; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xiii, 236. \$22.50.

One of the less appreciated legacies of the British raj to independent India was a vast literature of official documents, accurately observed, beautifully written, and logically organized. These papers have provided a mine of information for Indian historical scholars, who have produced from them endless monographs. This is one of the breed; not the best and not the worst.

Manilal Bose sets out to review the history of the North-East Frontier Agency from 1826 to 1947. Needless to say, this is an ambitious undertaking. Unfortunately, the subject is so diverse and unwieldy that he hardly provides an understanding of the development of the territory but rather skates lightly over the many facets of the subject and comes to some dubious conclusions. The least one can say is, "Not proved."

*Posa* (a form of revenue collection) and slavery come to mind as two of many possible topics that would make good monographs in themselves. Personal testimony of one of the ICS officers who served in Nagaland during the twilight of the British regime directly contradicts Bose. Yet, curiously, his work at times appears to be a distillation of official reports.

The style is marked by accented English. "There is no beard on men's faces and their hairs [*sic*] are straight," Bose writes (p. 19). And again, "Tea was an important item of import from China and a very good amount of gold and silver bullions [*sic*] had to be exported from Great Britain to pay for Chinese tea" (p. 176). The proofreading is careless.

An expert will already know for the most part what this volume has to say, and the nonexpert will be very confused by unexplained nineteenth-century Indian terms and ideas. The conclusion suggests that the British exploited the tribal people of the North-East Frontier, but the point is hardly sustained by the discussion. On balance, Bose would have done better not to publish this volume, which has all the stigmata of the revised dissertation and no exceptional virtue.

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ALAN FROST. *Convicts and Empire: A Naval Question, 1776–1811*. New York: Oxford University Press. 1980. Pp. xv, 240. \$45.00.

This is an interesting and valuable, if occasionally curious, study. Alan Frost, an Australian whose degree is in literature but who has moved into history, set out to "search for a less gratuitous beginning for Australia" (p. 182) than a British need to dispose of

convicts. He has found what he sought in the Anglo-French struggle for India, particularly in the "cold war" years that followed the Versailles Treaty of 1783. Maritime dominance in the East depended on bases and adequate supplies of naval stores to repair battle- or storm-damaged ships. The British, Frost points out, were not really well provided for in either respect. Bombay was on the wrong side of India, while reserve supplies of ship timber, spars, sails, and cordage had to come from Europe and were seldom adequate to the needs of a squadron on active service.

The difficulties Sir Edward Hughes had in keeping his ships seaworthy during his desperate fight to retain command of the Indian Ocean in 1781-82 amply demonstrated the fragility of the Royal Navy's administrative base in the eastern seas. By contrast, the French had a substantial base in Mauritius, and the Dutch, whom the French tried hard to turn into a satellite in the 1780s, were even better situated. Strengthening Britain's naval position in the East against France was therefore necessary, since the French, convinced that India was crucial to British power, clearly intended to renew their assault at the first opportunity. Frost's carefully researched account carries complete conviction on this point.

Frost finds the link between this strategic imperative and Australia in the search for an eastern source of naval stores under effective British control. One possible answer was to exploit Captain James Cook's discoveries. A colony might be planted in Australia's temperate climate. New Zealand flax grown there would provide sails; the pines of Norfolk Island to the east, masts and spars; and local agriculture, provisions. If the settlers were convicts, another problem—the overpopulated hulks in the Thames—could be solved as well. This scheme, pushed hard by such government advisers as Sir Joseph Banks (who had sailed with Cook) produced the 1786 decision to send the "First Fleet" to Botany Bay. Thus, concludes Frost, "the rag and bone shop of Australia's beginning was perhaps not so foul as we have for so long supposed" (p. 135).

If sending convicts to Australia for strategic reasons rather than simply to be rid of them makes the process less "foul," then Frost has proved his point (although the need to make such a point may strike some readers as odd). Apart from the appearance of protesting a bit too much, there are some awkward usages, like the archaic "marines" for "navies," and some characters move in or out of the story without every being fully identified. But, on the whole, it is a well-told story, and, if it is in some ways an extended footnote to Vincent Harlow's *Founding of the Second British Empire*, it is, unlike many footnotes, well worth reading.

RAYMOND CALLAHAN  
University of Delaware

ROGER C. THOMPSON. *Australian Imperialism in the Pacific: The Expansionist Era, 1820-1920*. Carlton, Victoria: Melbourne University Press; distributed by ISBS, Forest Grove, Oreg. 1980. Pp. xii, 289. \$32.50.

Roger C. Thompson defines Australian colonial policies in the Pacific as programs of British annexation or occupation of island territories. He feels that the Colonial Office was often cooperative but was sometimes overruled by the Treasury, always strong in Whitehall. The first chapter covers the Australian colonies' interests in the islands between 1820 and 1853. Persuasive activists such as John Dunmore Lang believed Britain's destiny was to occupy New Zealand and later envisaged an independent Australian Pacific empire. As early as 1854 Australians were calling on Britain to annex Fiji, and they played some part in the British takeover at Suva twenty years later. In the 1870s, Graham Berry and others pressed for an Australian "Monroe Doctrine" regarding the Pacific islands, and, beyond this, there was a fairly strong move to annex, or have Britain annex, all the unoccupied Pacific islands.

As to the premature annexation of New Guinea (April 4, 1883) by the colony of Queensland, under the directive of the premier, Thomas McIlwraith, Thompson feels that the fundamental reason for this action was not the German threat but the need to gain control of a pool of Kanaka labor to support the rapidly growing Queensland sugar industry. The author notes that fifty-five of seventy newspapers surveyed expressed approval of the annexation.

In 1883 James Service, premier of Victoria, advocated the annexation of the New Hebrides because of its supposed potential riches and the dangers that would arise were it developed as a foreign base. Australian concerns about the settlement of French *récidivistes* in that colony were considerable, in light of Australia's own earlier penal history. In 1885 Britain agreed not to approve any foreign annexation of those islands without consultation with the Australian colonies. The 1887 New Hebrides Convention largely confirmed this stand. The establishment of the Australian Commonwealth encouraged further interest in the Pacific islands on the part of Australian business, especially Burns Philp's trading and shipping interest.

During the First World War, Australian forces occupied Rabaul, beginning the takeover of German New Guinea—a source of irritation to Australia since the early eighties. The alliance with France during the war cooled the issue of the New Hebrides, except for a persistent Presbyterian lobby in Australia. As Thompson notes, Australian expansionism was over by 1920.

Queensland had given the main thrust for taking New Guinea, and Victoria was the driving influence to obtain a British influence in the New Heb-



rides. The author feels that Australian sub-imperialism paralleled similar developments in New Zealand and South Africa, involving a combination of economic ambitions, missionary hopes, and fear of foreign incursions into adjacent territories.

The research for this study is comprehensive, but the author's use of the language is impaired by a tendency to avoid using the article and to make nouns into verbs.

CHARLES S. BLACKTON  
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CLIFFORD GEERTZ. *Negara: The Theatre State in Nineteenth-Century Bali*. Princeton: Princeton University Press. 1980. Pp. xii, 295. Cloth \$18.50, paper \$5.95.

In this book the well-known anthropologist Clifford Geertz sharply criticizes the application of misleading Eurocentric models ("feudalism," "Oriental despotism," and so on) in interpreting indigenous Balinese political life. In four detailed chapters he discusses Bali's geographical and ecological situation; the Balinese system of social stratification; the nature of cleavage and alliance within the ruling class; the relations between rulers, villagers, and alien traders; and the extraordinary ceremonial density of Balinese religious life—both to reinforce his criticism and to develop a (fundamentally Weberian) model of his own, which he calls the "theatre state." He argues that the petty states (*negara*) of precolonial Bali were not primarily systems of governance, but rather organized spectacles designed to dramatize the ruling obsession of Balinese society—competition for pre-eminent status. The *negara* was not a mechanism for extracting surpluses from the peasantry nor an administrative tool for the coercive execution of policy, it was a continuing *tableau vivant* of Bali's cosmologically based conceptions of hierarchy. In a theoretical conclusion, Geertz suggests that his model is extendable to most of the "Indic" states of ancient Southeast Asia and that it provides an essential corrective to the overly coercive-administrative view of the state dominant in Western political theory since the sixteenth century.

Geertz's aim—to enrich Western political theory by confronting it with Bali—is both ambitious and admirable. And the book's four main chapters display their author's unmatched eye for telling detail, his gift for lucid exposition of the most intricate social organizations, and his ability to bring out hidden symmetries between different aspects and orders of Balinese life. Yet *Negara* suffers from weaknesses already intimated by its subtitle.

First, it is soon clear that nineteenth-century Bali really means pre-Dutch, traditional Ur-Bali; the date refers less to a specific period of Bali's history than to Geertz's resource base—Dutch records and

the memories of aged Balinese informants, not indigenous Balinese records of that era. The real nineteenth-century Bali was already deeply enmeshed in nineteenth-century world history: as Geertz himself notes, by 1830 the Dutch and British had ended Bali's profitable exporting of slaves; by 1849 the Dutch had begun the conquest of the northern part of the tiny island; and by 1859 Bali's largest import item was British-Indian opium. Reconstructing the *negara* from this Bali indicates the ahistoricism of Geertz's method.

Second, the arresting image of the "theatre state" is neither carefully explicated nor systematically applied. (Contrast the magisterial, systematic meticulousness of Louis Dumont's equally ambitious *Homo Hierarchicus*.) It ends up scarcely less arbitrary (and much less precise) than, say, "feudalism." Furthermore, the "theatre state" is not only not translatable into Balinese, but it turns upside down the Balinese idea of the relationship between their politics and their lively theaters: namely, that the latter are marginal to and derivative from the former. (It is curious that Geertz nowhere discusses Balinese theater.)

Finally, *Negara* also suffers from a florid, mannered prose that too often calls attention to the author rather than to its subject; and it contains too many thin "bon mot" generalizations for a text that takes other scholars to task for theorizing about what "they can not know."

BENEDICT R. ANDERSON  
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## UNITED STATES

ARTHUR MANN. *The One and the Many: Reflections on the American Identity*. Chicago: University of Chicago Press. 1979. Pp. xiii, 209. \$12.95.

In a series of eight pithy yet informal essays Arthur Mann dramatically depicts the historic interaction between ethnic pluralism and American nationality. In deftly elucidating a theme central to the American experience, Mann ranges eclectically across the whole gamut of American history from the Revolutionary generation to the present.

Not "ancestry, soil, church, soul, or folk" (p. 68), those familiar icons of nationalism, but an abstract and potentially universalistic idea inspired the unique legal and ideological charter that united the diverse peoples of a nation created *de novo* by a common adoptive American citizenship and identity. It is about this principle and its pluralistic ramifications, refreshingly explicated in the book's pivotal sections, that the discussion repeatedly returns. Aptly illuminating the differences between the United States and other multiethnic countries, Mann explores the various theories that have been designed, especially in the twentieth century, to re-



late the one to the many. He comments on the inadequacies of these theories, deplores the excesses of contemporary epigoni, and pays eloquent tribute to the nation's dual tradition of individual and group rights.

No less eloquent and piquant but less persuasive are the opening essays, "The Ungluing of America" and "Ethnicity Revived and Politicized." Somehow falling captive to the "white ethnic" rhetoric of the 1960s and early 1970s to whose practitioners Mann gives short shrift, he inadvertently legitimates their appropriation of the Manichean dialectic of an earlier era that he unequivocally rejects yet lets dominate his discourse. Virtually no attention is given to the profound shaking-up of the ethnoreligious no less than the ethnoracial substructures and verities of contemporary American society and the implications of these changes for the social order. The impact on American Catholics of the religious revolution of the 1960s, an upheaval without parallel in the history of Catholicism since the Reformation; the traumatic and delayed reaction of American Jews both to the Holocaust and to the plight of Israel; and the response by ethnic Southerners to the race revolution that was accompanied by the successive presidential candidacies of Lyndon B. Johnson, George Wallace, and James Earl Carter all need to be explained in the context of "the One and the Many."

Mann's keen perception that American diversity cannot be encapsulated in self-evident truths leads him to spurn not merely Israel Zangwill's melting pot but Horace Kallen's cultural pluralism and to mistakenly attribute to Kallen limitations and static metaphors that fail to do justice to Kallen's Jamesian sense of a pluralistic universe. Indeed, the father of cultural pluralism in his noted inaugural declaration in 1915 painstakingly elaborated on the dynamism of his central symphonic metaphor for an ever-changing pluralistic America. An active mediator among diverse Americans and the pre-eminent intellectual in the American Zionist movement in the early twentieth century, as well as a distinguished philosopher, Kallen spoke to diverse Jewish and other ensembles, as dissonant and discordant as only a free society and a modern symphony can contain. Clearly Mann's own pluralist faith has been implicitly shaped by Kallen's ever-expanding vision of the "American idea"—deliberately sketchy, ambiguous, and calculated to respond to the needs of Jews and all other Americans. Some will dispute whether that model will continue to be adequate.

MOSES RISCHIN  
San Francisco State University

SAMUEL S. HILL, JR. *The South and the North in American Religion.* (Mercer University Lamar Memorial Lec-

tures, number 23.) Athens: University of Georgia Press. 1980. Pp. xvi, 152. \$12.50.

Several decades will be required, remarks Samuel S. Hill, Jr., for scholarship in Southern religion to catch up with that in other areas of Southern history. A new vision of the unity of life in human society, he observes, should give the study of religion unusual importance. Few American historians, however, seem to have shared Hill's vision of the integral relation of religion to society. Rather they have tended to treat religion as a cultural sidelight. Hence, despite much interest in Southern history, we have no comprehensive or classic work in Southern religion.

Hill does not pretend to fill this need. These Lamar lectures do, however, provide a most valuable overview with which to start. He uses his comparative method skillfully to illumine the religious history of the South. The North serves more as a counterpoint. Without the South, he observes, there would have been no North. Drawing heavily on the available secondary literature, Hill's thoughtful analysis is filled with good sense and advances the scholarly discussion. This brief book also would make a fine supplementary assignment for upper-level courses in American history.

The lectures focus on three eras, 1795–1810, 1835–50, and 1885–1900, somewhat unengagingly called "Epoch A," "Epoch B," and "Epoch C." In fact Hill's analysis extends beyond these fifteen-year periods, so that really the eras he treats are roughly 1783–1820, 1820–60, and 1860–1917. Hill is very astute at sorting out the paradoxical relationships between North and South throughout these more extended eras.

In the early epoch before the Missouri struggle of 1820 there was hardly a "South" or a Southern consciousness. North and South were (in Hill's helpful but at first confusing terminology) "far" in the sense of having relatively little direct contact, but "close" in the sense of cultural unity. Religiously they differed only in that the Anglican heritage, compared with the Puritan-Calvinist, may have contributed to less social activism in the emerging evangelicalism of the nineteenth-century South. Throughout, says Hill, the irony of Southern religion has been an "ethos without ethic"—close cultural ties without a social ethic.

In the pre-Civil War era of conflict North and South paradoxically became "nearer" in the direct contacts and remained "close" in cultural traits. The slavery question, however, much accentuated the "ethos without social ethics" theme in Southern religion. Southern religion also was more homogeneous than was Northern, but similarities between the two were striking. These observations agree with other recent literature on the affinities between North and South in the antebellum era.

Only after the Civil War did the real cultural differences develop. While the North progressed, diversified, and became pluralistic, the South held tightly to the antebellum heritage, including their homogeneous, privatistic, but culturally supportive evangelicalism. Only in this era did the real Southern identity emerge. This identity then was projected back into images of the Old South.

Today, Hill concludes, these differences between North and South have largely disappeared. The religious and cultural heritages display fascinating differences left from the era of separation, but now the similarities are prevailing.

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RICHARD REINITZ. *Irony and Consciousness: American Historiography and Reinhold Niebuhr's Vision*. Lewisburg, Pa.: Bucknell University Press. 1980. Pp. 230. \$19.50.

This thought-provoking book argues that American historical writing attained a high level of maturity when, in the middle twentieth century, it emphasized irony in interpreting the nation's past. The author often overstates the claims for this perspective, and his own clear prejudices injure his case for the usefulness of the ironic point of view, but an engaging work results nevertheless.

Irony appears when discrepancy exists between intention and resultant action and when we can attribute that discrepancy to the fault, or weakness, however unconscious, of the actor. In a suggestive first chapter, Richard Reinitz, who died in 1979, advances his claims for the historian's perception of irony as a valuable intellectual tool. Irony has special value for American history because the fact and mythology of human freedom spawned illusions of innocence, virtue, and power that promoted actions contrary to their intentions, both in the home society and in the world at large. Reinitz values irony as a middle path between grand historical theories and narrow particularism and as a humanistic perspective that shuns both theistic and naturalistic determinism; with its interest in ideas, values, and intentions and in their consequent effects, it merges intellectual and social history. Irony preserves for the historian a measure of moral judgment on the past without recourse to simple Manicheanism. But above all, and in a manner that unfortunately dominates his efforts, Reinitz tries to urge irony on the "radical historian," whose purpose is to expose the inadequacies of the old order.

Reinitz briefly discusses several European historians—Hegel, Marx, de Tocqueville, and Weber, among them—before exploring the uses of irony by Parkman, Hildreth, Henry Adams, Becker, and

Parrington in early American historiography. These discussions set the stage for Reinhold Niebuhr, the most profound theorist of the subject. The long discussions of Niebuhr, however, become a running quarrel between the author and his subject. Reinitz charges that Niebuhr's ardent Christianity impaired his humanistic view of irony and that his patriotism compromised his historical view of it. Whereas it could be said that it was precisely Niebuhr's Christianity that made his ironic view of human nature so compelling, even Reinitz's charge of excessive chauvinism against Niebuhr is unfair. The author throughout exhibits an a priori anti-Americanism and faults all historians who do not severely censure the United States. He forgives Niebuhr in his later writings when the theologian criticized American involvement in Vietnam. Then too, almost tautologically for Reinitz, Niebuhr's perspective became authentically ironic.

Reinitz's best sections analyze three American historians of the "consensus" school—Richard Hofstadter, Daniel Boorstin, and Louis Hartz. Here he brings better balance to his assessments. He praises Hofstadter as one who used ironic perspective to describe the American political tradition, and its marked continuity, while also remaining critical of that tradition. Boorstin, on the other hand, wrote history that avoided irony altogether. Dominated by pragmatic and workaday values, Boorstin's Americans never took abstract idealisms seriously, so no disparity of thought and action evolved. Reinitz's prejudices perhaps yield their own irony. He wants American historians to use irony to expose national ideals for the shams they are, but when he confronts Boorstin, who dismisses these sentiments from causal influence, Reinitz charges Boorstin with caricaturing Americans as petty and trivial, crudely pragmatic, and devoid of noble aspirations. This charge might be tolerable enough were it not true that Reinitz nowhere admits that any good came of these pretensions. In the end he gives his blessing only to the recent crop of New Left historians in whom ironic history fulfills its highest mission in anti-Americanism.

One could say more for this book were it not so very badly edited. Indeed, the mechanics of English prose yield to the computer; ellipses are constantly broken into two lines and in one case a line begins with quotation marks and a footnote that complete the sentence in the line above (p. 174). Some sentences make no sense whatsoever.

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GEORGE M. FREDRICKSON. *White Supremacy: A Comparative Study in American and South African History*.

New York: Oxford University Press. 1981. Pp. xxv, 356. \$19.95.

George M. Fredrickson's attempt to make a historical comparison between South Africa and the United States is an exercise much akin to a comparison between apples and pineapples or between horses and cows. The feasibility of the enterprise cannot obscure the intrinsic incongruity, and eventually the contrasts outweigh the commonalities. Indeed, the author makes this admission with embarrassing regularity throughout *White Supremacy*. And despite its readability, this is a very unsatisfactory book. It adds very little to the general histories of either South Africa or the United States. It contributes only marginally to an understanding of the genesis of race relations. And it violates the most fundamental concept of comparative history. These are, admittedly, strong indictments against a work by a distinguished American historian—especially when the work is presented to the public garnished with the most glowing encomiums from a galaxy of equally distinguished professors—but they can be easily substantiated.

Basically Fredrickson takes a linear view of the historical development of both South African and United States societies. The analytical narrative begins in 1600, with "settlement and subjugation" in both regions, examines the rise of "racial slavery in the South and the Cape," describes the development of "race mixture and the color line," political conflicts between the separate emergent states, and the expansion of patterns of racial discrimination with its ensuing conflicts. It concludes with a discussion of "segregation in South Africa and the South," which, congruent with the tenor of the preceding chapters, confesses that "despite some superficial similarities, therefore, the differences between Jim Crow and 'native segregation' or 'separate development' are too great, in terms both of underlying structures and patterns of historical development, to sustain an elaborate comparison" (p. 250). Supporting the chronology is a list of important dates in the histories of the two states. The bibliography is informed and up-to-date. While the major outlines of the narrated histories of both the United States South and South Africa are acceptable, there is ample room for basic differences. Debatable points include the relationship between slaveholding and apartheid (p. 238) and the assertions that America is the "world's richest nation" (p. 5); that "miscegenation is likely to be especially extensive where the predominant relationship is between master and slave" (p. 95); that Africans were enslaved because of "their cultural and legal vulnerability" (p. 70); that Edward Long was a "Jamaican physician" (p. 142); that humanitarianism was a major factor in British slave emancipation (p. 186);

that the Morant Bay (Jamaica) Rebellion affected English thinking on black civil rights; that South Africans were less guilty of racism than of "intense ethnocentrism" (p. 194); and that in the South "workers were segregated without being treated differently by employers" (p. 215).

The introduction trying to explain why the phrase "white supremacy" is preferable to "racism" reveals more sophistry than sophistication. Essentially, what the author says is that since no one today will admit to being a racist, then the term must be discarded in preference of the "relatively neutral" phrase "white supremacy." On this basis, it is disconcerting at best and deliberately insulting at worst to note the frequency with which the author employs "Negro" and "Negroid" as synonyms for black. The opposite of white surely is nonwhite or black. In addition, maudlin attempts to preach against the "notion that white Americans have a kind of moral superiority over white South Africans" (p. 247) is more histrionics than history.

The fatal flaw in this book, however, is that it completely fails to establish the comparability of the two cases studied. No hard figures are given for populations and groups, nor any relationship of man to land. The human ecology is never clearly or forcefully drawn. Even the concept of "free coloreds" or miscegenation is hazy. Fredrickson appears uncertain at times whether he should compare the amorphous South African nonwhite group legally designated as "coloreds" with the North American nonwhites as a whole, the Afro-Americans, or the American Indians. He seems unaware of the somatic norm variations between Africans and Asians. And he apparently is not convinced that systadial comparisons offer greater efficacy than synchronic comparisons. Altogether then this book is neither a rewarding nor a pleasurable reading experience.

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THOMAS A. BAILEY. *The Pugnacious Presidents: White House Warriors on Parade*. New York: Free Press. 1980. Pp. ix, 504. \$17.95.

This latest work of an eminent historian, who, by his own count, has published about twenty books, twenty revisions, and more than a score of articles, is not the product of extensive research. It analyzes each president's militancy or pugnacity in dealing with foreign nations but with digressions into domestic disturbances. There is an undocumented chapter, rarely exceeding seventeen pages, for each incumbent and a fortieth called "Pugnacity in Perspective." A bibliography averages three titles, mostly biographies, for each man. The author has

reread the compilations of presidential papers and has drawn upon his own *Presidential Greatness* (1966), his perennially popular textbook on American diplomatic history, and a sixty-two page House committee print of 1970 entitled *Background Information on the Use of United States Armed Forces in Foreign Countries*.

The style is vintage Bailey with such captions as "The Tussle with Tripoli," "Madisonian Mud-dlings," and "The Fulfillment of Fillmore." He cannot resist writing that in 1905 the Japanese began to run out "of men and yen" and seemed to be "getting too big for their kimonos" or that in the U-2 incident the Americans "had stooped to snoop; they had lied, denied, and defied" (pp. 33, 49, 165, 328-29, 427).

Despite the claim that the book will demolish myths and surprise readers, specialists will find little new. They may disagree with overly condensed treatments (Polk and the notice on the Oregon Treaty) or with interpretations (Wilson's diplomacy), but the chief flaw is an inability to define and measure pugnacity. Can one distinguish "between aggressive pugnacity and defensive bellicosity" and then conclude that "Lincoln's bellicosity was defensively aggressive" (p. 205)? The author argues that the United States acted as a world power long before the supposedly watershed year of 1898, citing both the constant use of the navy to protect American citizens in distant lands and presidential references to the nation as a great power. But when he notes that there were eight warships in Asian waters during the Sino-Japanese War of 1894-95, he fails to say that the president and the secretary of state each insisted that that war imperiled no basic policy of the republic. Surprisingly, he never mentions the War Powers Act, passed over Nixon's veto and called into question under Ford. There are over a dozen wrong dates and typographical errors and more factual mistakes than one would expect from so careful a scholar. Guam is not a South Pacific island. Wilson's brain was not fogged with sleep on April 21, 1914, when he ordered the seizure of the custom house at Veracruz. Bryan did not favor an arms embargo in 1915. The Kellogg pact did not "outlaw" war. Eisenhower's statement about the outcome of an election in Vietnam referred to a contest between Ho Chi Minh and Bao Dai, not between Ho and Ngo Dinh Diem.

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ARTHUR A. STEIN. *The Nation At War*. Baltimore: Johns Hopkins University Press. 1980. Pp. xii, 151. \$12.95.

This book, by a political scientist at UCLA, brings the analytical techniques of the social sciences to

bear on questions frequently (and often sloppily) addressed by historians: do wars increase or decrease domestic cohesion, concentration, and inequality?

Arthur A. Stein sets out four hypotheses. Social cohesion in war, he postulates, increases as a function of the strength of the external threat and decreases as a function of the depth and duration of mobilization. Concentration of production increases to the extent that wartime mobilization exceeds the peacetime full production capacity of the economy. Inequality is lessened as a function of the mobilization process.

Following good social science procedures, Stein next seeks the relevant empirical data to test these propositions. Least satisfying is his treatment of the factor of threat, where he relies heavily on historians' retrospective appraisals of the actual danger to national security in a given war, rather than attempting to assess contemporary perceptions of such danger. Many of his data series are predictably, though judiciously, chosen. The extent of mobilization, for example, is measured by examining increases in military personnel. Other empirical measures are more ingeniously conceived, as in Stein's discussion of labor participation rates (on the theory that labor income is more evenly distributed than other forms of income) to demonstrate decreasing income inequality in wartime.

Stein's conclusions produce few surprises. His evidence tends to confirm his first two hypotheses about threat and cohesion, to confirm with some qualifications his third hypothesis about concentration, and to confirm rather strongly his fourth hypothesis about inequality. The blandness of these results is often matched by observations of bone-rattling banality along the way. "[T]he attack on Pearl Harbor confirmed the existence of a threat to the United States," the author pronounces, an item he deems sufficiently remarkable that it must be italicized (p. 32).

There is rigor and imagination in Stein's analysis, but in the end he does not appreciably advance our historical understanding of war's effects. (This may be in part because he appears to have systematically ignored most of the standard historical literature on the topic.) One might even ask if he has posed the most significant questions about the domestic consequences of war, or only those questions most susceptible to quantifiable answers. Readers of this book receive no instruction about war's effects on social and political values, or on the distribution of social power, and receive precious little enlightenment about the permanency of the wartime effects that are demonstrated.

No sense of historical texture or context informs these pages. To reduce the different effects on social cohesion produced by World War II and Vietnam



to a calculus of the varying degrees of threat and mobilization manifested in those two wars is to turn a blind eye to moral and political issues too important to be cast out of the discussion simply because they cannot be adequately quantified.

At its best, this book amounts to a codification of common sense. At its worst, it amounts to an unacceptable distortion of some of the major facts of life for American society in this century.

DAVID M. KENNEDY  
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ROBERT V. HINE. *Community on the American Frontier: Separate but Not Alone*. Norman: University of Oklahoma Press. 1980. Pp. xii, 292. \$12.50.

In *Community on the American Frontier* Robert V. Hine asks whether the frontier was congenial to the establishment or the maintenance of community. In general his answer is no, but along the way he gives accounts of some remarkable achievements of community. The problem is that so little is achieved in the way of explaining the rule and its exceptions. What kind of a factor is the frontier in this process? What qualities external to the frontier when brought there are likely to generate community? These obvious questions are lurking in Hine's text, but they do not give form to the book; indeed, it lacks an interpretive structure. It is organized instead according to community types, roughly chronological in their appearance in the American experience—though chronology is largely ignored within each chapter. The communities to which he devotes a chapter are: Puritan towns, the wagon train, mining camps, isolated farms, country towns, rancheros, ethnic colonies, and cooperative colonies.

Neither community nor frontier is precisely defined, hence any causal statements are problematic. The aspects of "frontier" that are specified as corrosive in respect to each of the settlements vary, but the most consistently mentioned qualities are those of mobility and opportunity. Unfortunately for the explanatory power of the notion of frontier—and for the book itself—these are not strictly frontier phenomena. Certainly, nineteenth-century cities can be described equally well as the locale of mobility and opportunity.

Community is generally defined by Hine in terms of a commitment to and of a whole. His working definition of community is one that John Winthrop would recognize. But such a definition is too restrictive for the nineteenth century, and certainly it discourages Hine from exploring forms of community that fall short of comprehending a social whole. Out of numerous possible examples, let me mention two. When he argues (p. 168) that the Southwestern ranchos did not achieve community fully because

the cowboy was not invited into the big house, I cannot help remembering that community creates and requires "others." What is being described here is not so much the failure of community as one of the boundaries of community. Elsewhere he presents as evidence of the weakness of community in mining towns the tendency of miners to "congregate [within the town] according to their roots" (p. 76). It seems that a simple yes and no judgment is being offered while a far more fruitful question is being avoided: how these smaller units of community relate to the larger problem of the organization of social order.

At several points in the book we meet a very provocative and compelling idea that one wishes he had pursued systematically. Hine observes that place, memory, and physical form are key elements in the construction and maintenance of community. A place nourishes memory and memory invests space with symbolic meaning. Without these qualities community cannot flourish. Although it was published too late for him to use it, this notion of Hine's gains powerful support from the recently translated classic of Maurice Halbwachs, *The Collective Memory* (1950, 1980), and it opens a rich vein of cultural analysis for historians.

THOMAS BENDER  
New York University

DAVID GRAYSON ALLEN. *In English Ways: The Movement of Societies and the Transfer of English Local Law and Custom to Massachusetts Bay in the Seventeenth Century*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1981. Pp. xxi, 312. \$27.00.

Intellectual historians frequently emphasize the connection between Tudor-Stuart England and seventeenth-century America, but social historians describing the "reality" of colonial life tend to ignore English antecedents. The assumption behind this neglect is that the physical and economic attributes of the New World, plentiful land, isolation, increased opportunity, geographic mobility, and so forth, produced a new social order that owed more to the newness of America than to the traditions of England. David Grayson Allen's extraordinarily detailed and well-researched study of the transition of culture from England to five Massachusetts towns challenges this neo-Turnerian view of the social historians. His thesis is quite simple: English local life was diverse in its patterns of wealth distribution, leadership, agriculture, and landholding; Englishmen who moved to Massachusetts recreated the worlds they had known; hence, Massachusetts towns differed widely in their social patterns, and these differences derived from the place of origins of

the settlers. Settlers from the West Country, Yorkshire North Country, and East Anglia created towns in Massachusetts as different in social structure as those three regions were in England. The thesis is simple but important, and the method of demonstrating it is sophisticated and for the most part successful.

In reading *In English Ways*, one is struck by how much colonial historiography has changed since the publication in 1963 of the only other community study on New England to be thoroughly researched in English sources, Sumner Chilton Powell's *Puritan Village*. Powell's charming qualitative analysis gave his book the literary grace to win a Pulitzer Prize: Allen's quantitative evidence and his persistence in hammering home his thesis will appeal to far fewer readers. Yet, for all its grace, *Puritan Village* did not make a lasting contribution to our understanding of the colonial period, and *In English Ways* probably will. Other recent scholars have argued that Puritan towns comprised no monolith and that size, location, economic function, and settlement date distinguished them from each other. Allen adds the settlers' place of origin to this list of differentiating factors.

Not surprisingly in such an ambitious book, some problems bedevil the analysis. In a few places the connection between old and New England breaks down and Allen tries artificially to maintain it. For example, he argues that Rowley Parish in England was a hotbed of Puritan dissent, yet, Rowley Massachusetts "became a traditional society of orderly established ways" (p. 168). Why would a radical English community spawn a conservative New England town? Allen answers that "the youthful disdain for orderly established ways" in Rowley Parish "must have been of negligible significance" (p. 168). This is a clear case of a thesis being strained instead of sustained by the evidence. One of Allen's secondary theses, that the first generation of Massachusetts towns enjoyed near autonomy, is not completely convincing; he does not deal with David Koenig's sophisticated argument to the contrary in *Law and Society in Puritan Massachusetts* (1979). The organization of the first half of the book is too mechanical: three successive chapters each deal with an English parent community and its Massachusetts offspring; this approach may be effective in demonstrating the thesis, but it stifles reader interest. And, finally, Allen deals in most places with the terms Massachusetts and New England as if they were interchangeable: they were not, and a book that emphasizes diversity should not fall into this error.

Books without problems, however, do not exist, and the problems indicated are but a slight counterweight to the solid achievements of *In English Ways*. It significantly advances our knowledge of

English local life, Massachusetts towns, and the transmission of culture across the Atlantic.

BRUCE C. DANIELS  
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JOHN E. FERLING. *A Wilderness of Miseries: War and Warriors in Early America*. (Contributions in Military History, number 22.) Westport, Conn.: Greenwood Press. 1980. Pp. xiv, 227. \$25.00.

This book might be more appropriately subtitled "Violence and Militarism in Early America." John E. Ferling has not aimed at writing a comprehensive narrative about war and warriors for that long and diverse period. Rather, he has attempted in six related essays to examine the impact of warfare on early American society with special attention to the Revolutionary era. His recurrent theme is that white Americans, from the first day of settlement to the adoption of the Constitution, were a peculiarly violent and militaristic people. They gloried in their military heritage, inculcated their children with military values, and readily accepted war as an agent of change. Ferling concedes that the colonists' martial spirit did not contribute to the deterioration of relations between America and Britain in the late 1760s and the early 1770s, but he does suggest that it "rendered most colonists ready, even anxious, to challenge the parent state" on the eve of the Revolution (p. 200) and enabled them to fight the ensuing war vigorously and effectively.

This "colonies militant" thesis, as it might be styled, is based principally on some of the ideas about war and society that have been put forward in recent years by the new breed of American military historians, John Shy in particular. Early American military attitudes are explained as deriving in part from the violent European attitudes of the wars of religion, which were brought to the new world by the earliest settlers and became "frozen" in the colonial culture (p. 199), and in part from the new experience of unrestrained warfare with the Indians in the American wilderness. The colonists are portrayed as being aggressively expansionist and as favorable, rather than averse, to standing armies and offensive wars when they served their ends.

To augment and enlarge the ideas borrowed from the new military historians, Ferling has drawn on social, political, and even art historians and has included numerous quotations from printed primary sources. The latter, unfortunately, have been misused in several places in such a way as to obliterate some of the subtleties and complexities inherent in the events. For example, in discussing the undeniably violent tenor of early American warfare, Ferling accepts many colorful atrocity stories at face value. Some of the worst accusations that Indians

and whites, loyalists and patriots, made against one another appear without the requisite pinch of salt. They prove the existence of an extremely violent rhetoric but do not really substantiate Ferling's exaggerated claim that "every colonial war brimmed with atrocities" (p. 92).

The book, in brief, contains some provocative ideas with important implications for both past and present-day American society, but it must be said that they have not been argued either closely or clearly. Repetitions, contradictions, extraneous digressions, non sequiturs, and minor factual errors are found throughout the work. Ferling, however, has been uncommonly assiduous in surveying the relevant literature, and many scholars will find his extensive footnotes and bibliography very useful.

PHILANDER D. CHASE

*Papers of George Washington*

SARGENT BUSH, JR. *The Writings of Thomas Hooker: Spiritual Adventure in Two Worlds*. Madison: University of Wisconsin Press. 1980. Pp. x, 387. \$19.50.

MASON I. LOWANCE, JR. *The Language of Canaan: Metaphor and Symbol in New England from the Puritans to the Transcendentalists*. Cambridge: Harvard University Press. 1980. Pp. x, 335. \$20.00.

Literary analysis of Puritan rhetoric has often produced works of importance for historians of New England. Now Sargent Bush, Jr., and Mason I. Lowance, Jr., continue that tradition with a detailed examination of Thomas Hooker's thought and utterance and an ambitious history of New England's language of metaphor from John Cotton to Henry Thoreau.

Bush's work fills a long-felt need. Despite fame in his own generation and a modern reputation as a father of New England, Hooker has remained an uncertain influence in Puritan history, and special difficulties have always surrounded his written works. Unlike many of his brethren, Hooker was seldom eager to publish. Consequently, our texts have often been reconstructed from anonymous auditors' shorthand notes, and unknown dates and circumstances of composition and continuing questions about the definition of his literary canon have long troubled Hooker scholars. Evaluating Hooker has also required an exact appreciation of his contribution. Without ambition as statesman or controversialist, he spoke as preacher, pastor, and shepherd.

Recently these problems have begun to be effectively dealt with in important articles by Everett Emerson, George H. Williams, Keith Sprunger, and Norman Pettit, a new biography by Frank Shuffelton, and a critical edition of Hooker's early writings

by Williams, Pettit, Winifried Herget, and Sargent Bush, Jr. Now Bush has provided us with a survey of Hooker's recoverable oral and written literature together with a perceptive commentary. For the first time we have a reliable guide to Hooker's total literary achievement, a detailed explanation of his system of instruction, and a critical evaluation of the place of his writing in Puritan history.

A basic division marks Bush's management of the Hooker material: he first treats the "occasional works" that reflect Hooker's historical career, while the second and larger part of the book deals with the "timeless works," the spiritual counsel by which he guided his people along the pathway of salvation. Significantly, the "occasional works" were almost all elicited by English or Dutch, rather than New England, controversy and define his partisan Independency. Bush sets all of these writings in historical context, resolves doubts about Hooker's aims and arguments, and suggests probable relations to later New England developments. Necessarily, some of this discussion is speculative, but it is usually persuasive. Perhaps the least convincing is his treatment of *The Saints Dignitie and Dutie*, which he thinks is Hooker's response to the Antinomian controversy in seven sermons probably delivered in Boston. The evidence seems too slippery for any conclusion, and one clue on which Bush relies—that Hooker several times refers to his congregation as living in a "city"—might argue instead against a Boston setting. (Even in 1716 Cotton Mather fixes "upon the Town, the Name of, a City" only to "animate the Inhabitants, to consider more for the Good of it.")

The more elaborate part of this study is the examination of "practical divinity." Beginning with *The Poor Doubting Christian* (1629) and continuing systematically through the many treatises of the 1630s, Hooker produced Independency's most impressive corpus of minute pastoral diagnosis and counsel. To analyze this dissection of the Puritan personality in all of its theological and psychological complexities the author skillfully reassembles the intellectual environment in which Hooker's curative mechanisms were formed. Renaissance faculty psychology and preparationist theology are major components, but even Baconian and Platonic influences at old English Cambridge are carefully integrated. As a result, we are provided with the most satisfying explanation to date of the way in which scholarship, rare psychological insight, superb command of language, and humanity in treatment mingled to sustain Hooker's appeal. Yet curiously, this detailed literary analysis also seems to present a qualified view of Hooker in which his own evangelical intensity and sense of subordination to God's word appear overshadowed by his mastery of didactic and rhetorical strategies.

Consistent with his approach is Bush's con-

cluding suggestion that Hooker's monumental portrayal of the questing pilgrim created a heroic type, a legacy for later American literature, apparent in "Cooper's *Leatherstocking*, Melville's *Ishmael*, Whitman's 'Self,' and Twain's *Huckleberry Finn*" (pp. 312-13). Establishing a continuity between Puritan rhetoric and the succeeding two centuries of American writing is much more central in Lowance's *Language of Canaan*. This work is a history of New England's use of metaphor and symbol derived from the richly developed Puritan "science" of Biblical exegesis and evolving through Enlightenment and Great Awakening to the nineteenth-century "American Renaissance." The study builds on the interest in typology current among critics of seventeenth-century literature and more especially on Lowance's own essays on Samuel and Cotton Mather and Edwards's *Images or Shadows of Divine Things*. Yet in this work the author attempts a more systematic historical analysis than previous investigations.

Lowance begins with a detailed examination of the principles of Reformed scriptural interpretation and their appropriation by Puritans of the migration age. The American mode of symbolic expression, he argues, was primitively set by two traditions of commentary: the one stressed literal interpretation, used the English apocalypticists, Mede and Brightman, and eventuated in the millennial anticipations of John Cotton and Increase Mather; the other, legitimized by commentators struggling with the difficult Song of Solomon, emphasized allegorical meanings and eventually bore fruit in the poetry of Edward Taylor. Lowance's chain of descent leads through Cotton Mather and Edwards to Joel Barlow and the Transcendentalists, but there is substantial discussion of lesser writers as well. The book's underlying theme is that the prophetic language was progressively modified so that it came to be an instrument, not only for understanding scripture and history but also for reading nature; after Edwards "writers were able to explore the world of spirit that lay around them throughout all nature rather than in the sanctioned confines of biblical expression, eschatological symbol, and divine event" (pp. 7-8).

Despite the claimed historical range, the book is attentive primarily to the seventeenth century; a final chapter leads from Edwards to Thoreau. Treatment of the immense literature tends to be selective—Cotton and the Mathers, for instance, but not Hooker or Williams—and in the early period is broadly transatlantic, including discussions of Samuel Mather (a chapter), Flavel, Keach, and other English Puritans. Yet the study provides us with important new understanding of the structure and meaning of Puritan expression and of their place in shaping that tradition of American writing that

tends to find truth in reading the world emblematically.

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CONRAD CHERRY. *Nature and Religious Imagination: From Edwards to Bushnell*. Philadelphia: Fortress Press. 1980. Pp. x, 242. \$12.95.

Nature fascinated early Americans, and the present volume treats this fascination in New England theology from the early eighteenth to the middle of the nineteenth century against a background of Anglo-American social and intellectual change. The book also discusses the artistry preachers employed to convey their message.

Conrad Cherry maintains that a symbolic-imaginative response to nature appeared in Jonathan Edwards, subsequently declined in a period of moralism, legalism, and rationalism, and later reappeared in the religious romanticism of Horace Bushnell. Throughout, tension between religious didacticism and religious symbolism persisted.

Edwards was indebted to the Puritan tradition of typology, but he emphasized symbolic interpretation. For Edwards, physical nature was an image of divine things; man knows God through symbols. Believing that a religious understanding of nature's images requires conversion, Edwards's sermons deliberately used images to terrify consciences and win souls.

New Divinity theologians shifted the focus to nature's precepts; physical nature embodied a set of instructions on man's duty to God. Bishop Butler, Archdeacon Paley, and the Common Sense philosophers persuaded Americans to view natural and moral government as constituting one system and design in nature as conducive to human happiness. Both the New Haven theology and Channing's Unitarianism emphasized the theme of moral government and nature as a code of laws requiring religious duty.

Bushnell introduced romantic innovations while retaining ties to his heritage. The greatest influence on him was Coleridge, whose theory of the imagination marked a major transition in American thought. For Bushnell religious language is symbolic expression and Christ is the chief metaphor of God's truth. Nature and supernature constitute one system; redemption comes when the illuminated imagination recreates the soul in God's image. Though Bushnell searched for images that would prompt sinners to respond in faith, his stress on organic personal growth robbed his sermons of revivalistic urgency.

Cherry shows the implications of his study for the



larger culture by observing that American Protestantism has resolved the continuing conflict between religious moralism and religious symbolism by emphasizing the former. This penchant for moralism has led to sentimentalism and self-righteousness. Yet there is danger in a Christianity that favors symbolism, as evidenced in the lack of passion for eliminating specific social evils in the theologies of Edwards and Bushnell.

*Nature and Religious Imagination* deals carefully and sensitively with an important subject, its focus on symbolism and the metaphoric mode is one of its most valuable contributions, and it exposes the roots of national traits still with us. Readers will find the book richly rewarding.

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K. G. DAVIES, editor. *Documents of the American Revolution, 1770-1783*. Volume 19, *Calendar, 1781-1783, and Addenda, 1770-1780*; volume 20, *Transcripts, 1781*; volume 21, *Transcripts, 1782-1783*. (Colonial Office Series.) Kill-o'-the-Grange: Irish University Press; distributed by Biblio Distribution Centre, Totowa, N.J. 1978; 1979; 1981. Pp. vii, 541; vi, 309; vi, 276. \$60.00; 50.00; 50.00.

The publication of these three volumes of *Documents of the American Revolution* completes a bold project launched a decade ago that constitutes a landmark for students of the American Revolution. Since Benjamin Franklin Stevens undertook to reproduce twenty-five volumes of facsimiles of documents in European archives related to the American Revolution nearly a century ago, many significant projects for tapping the riches of the Public Record Office have been undertaken, but none equals K. G. Davies's *Documents* in scope or utility. Here nearly 28,000 documents have been expertly calendared and described, and of these over 1,800 are printed *in extenso*. The whole has been issued in seven three-volume units, each consisting of a calendar volume covering two years followed by two volumes of transcripts selected from the most significant documents calendared.

The subject of the work is imperial administration; and because its focus is the "colonial office" during the years 1770-1783, its major theme is the breakup of a vast empire. Davies's stated purpose is simply to "recreate the situation as regards information in which the Secretary of State for the American Department found himself." This preoccupation with the American secretary has important consequences that often subtly shape the scope of the project; thus certain classes of West Indian records are excluded for the years before the

outbreak of war but included in later volumes when Caribbean operations become central to Lord George Germain's plans to crush the American rebellion. Similarly, Germain's priorities affect the selection of documents to be transcribed, although this is but to state the obvious since the editorial purpose of any "select" documentary project will determine the shape of the product. Fortunately, Davies exercises his editorial prerogatives with superior judgment and keeps the reader informed of all significant factors related to his selection principles. Indeed, in explaining his principles and practices Davies is perhaps overly apologetic, undoubtedly out of frustration that he was unable to offer a truly comprehensive work equal in scope to the full range of materials available in the Public Record Office. It is clear, nevertheless, that this work needs no apologies, for, if compromises were dictated by considerations of cost and time, such grounds are legitimate ones, and Davies's able and lucid explanations of the most important of these merit respect.

Davies has also been sensitive to the implications of a number of anomalies of the Colonial Office archive and in his masterful introductions to the calendar volumes has shared his insights into the consequences of a number of these. Thus he takes pains to explain the conspicuous "rise in the volume of British interdepartmental correspondence respecting the colonies beginning towards the end of 1771 and gathering speed in 1772," as well as "the archival inflation of 1775-1776." The former is signaled in the American department's opening up new and separate files of Admiralty, Post Office, Treasury, Customs, and Ordnance Office correspondence: "In practice archives seldom or never burgeon without an assignable cause," Davies explains, "and this case is no exception." Editorial discussions of the nature and distribution of the American secretary's correspondence and lists of the "Principal Royal Officers in North America" and of the sources of the documents described, which accompany each calendar volume, are also very useful.

The introductions to the transcripts consist primarily of summaries of the major events and issues covered in these volumes, which demonstrate Davies's mastery of his subject. Indeed, these brief editorial essays should be required reading for all serious students of the American Revolution as they provide both an excellent panoramic view of the period and numerous special insights into the world of the policy makers working at Whitehall.

This high praise is not meant, of course, to suggest that Davies is above criticism or that warnings are not in order for the unwary user of the work but only that on balance the editor has responded splendidly to many difficult challenges. Some readers will be puzzled, for example, that a work focusing on the American department begins in 1770

rather than with the creation of the American secretaryship in 1768. The decision to exclude a number of West Indian documents that appear little different from many included for Quebec, Nova Scotia, and Newfoundland will disconcert others. And the treatment accorded the intercepted letters listed in volume 10 that "have no more than a tenuous connexion with the main body of Colonial Office documents" bears the marks of an overzealous editor bent on telling a story rather than permitting the sources to speak for themselves. Finally, the degree of modernization employed in transcribing documents selected for printing *in extenso* is difficult to justify in a work clearly intended for scholars.

But such reservations merely underscore the truth that documentary editions are primarily tools to be used by craftsmen who understand the inherent limitations of all such works. Readers who use them uncritically or who slavishly defer to even the most accomplished editors do so at considerable risk. This edition is a high-quality tool that will be put to innumerable uses by scholars having diverse interests, including many unrelated to the workings of the American department, and will make practicable a number of investigations that hitherto would have required more time to complete than most scholars could afford to invest. It does not cover, of course, many rich Public Record Office groups of documents, such as Admiralty, Exchequer and Audit, Treasury, and War Office papers, or related classes "Acquired by Gift, Deposit or Purchase," such as the Carleton Papers (P.R.O. 30/55) and Cornwallis Papers (P.R.O. 30/11). But in the future, historians of the American Revolution will have no excuse for ignoring the riches of the Colonial Office, in part because microfilm of most of the material described by Davies can now either be purchased from the Public Record Office or is available on interlibrary loan from the Library of Congress. We can only hope that greater familiarity with the menu Davies has given us will stimulate future efforts to make other Public Record Office materials equally accessible.

PAUL H. SMITH  
Library of Congress

WILLIAM WINSLOW CROSSKEY and WILLIAM JEFFREY, JR.  
*Politics and the Constitution in the History of the United States*. Volume 3, *The Political Background of the Federal Convention*. Chicago: University of Chicago Press. 1980. Pp. xii, 592. \$27.00.

The first two volumes of William Winslow Crosskey's mammoth study of the Constitution appeared in 1953. Never had a publication of this sort so thoroughly amazed the critics. In both historical and legal journals, shocked reviewers saluted the

enormous effort and overwhelmingly condemned both the scholarship and the audacious findings.

Dedicating the work to Congress, "in the hope that it may be led to claim . . . the powers justly belonging to it," Crosskey purported to demonstrate that the "actual, historic meaning" of the Constitution was radically at odds with the understanding received by the twentieth century as a consequence of Jeffersonian distortions, the inactivity of early national Congresses, and the errors of the Supreme Court (vol. 1, p. vii). By recovering the ordinary meanings of words and concepts from a range of primary materials, Crosskey claimed, it had been possible for him to create a "specialized dictionary" of eighteenth-century usages. Employed in conjunction with standard rules of legal interpretation, this dictionary revealed the framers' lost intent. What they intended, he maintained, was to establish a unitary, sovereign national government with plenary powers, in particular, over the full range of the nation's economic activities, not the limited, federal system that the nineteenth century came to know. The philological or textological exploration of volumes 1 and 2, Crosskey promised, would be followed by supporting studies of the political background of the movement for constitutional reform and of the convention itself.

Volume 3, prepared for publication by William Jeffrey, Jr., from a typescript completed by Crosskey before his death, shares all the characteristics of its predecessors. Grounded, as they were, on research that ranges widely, though selectively, through contemporary newspapers, legislative records, and superseded editions of collected writings but which systematically dismisses or neglects the most important secondary sources, it is a tour de force of scholarship subordinated to a point of view.

After a preliminary section on early revolutionary sentiment in favor of a vigorous central government, the volume argues that the thrust toward constitutional revision during the 1780s was a broad-based movement, resisted principally in the South, to confer on Congress plenary power over intrastate as well as interstate and foreign commerce. Head-on challenges to prevailing views abound. Mention of two of these should sufficiently suggest the unconventional nature of the thesis. "The truly fathering influence" in the production of a constitutional convention "was the political astuteness and adroit maneuverings of a handful of men of the North," who manipulated the controversy over the Jay-Gardoqui negotiations so as to convince reluctant Southerners that they must accede to the kind of central government the North desired or face the Spanish and their slaves alone (vol. 3, pp. 355, 353). The Virginia Plan of 1787 "really grew," not from the research of James Madison, who is once again accused of tampering with the record, but from the

desires of Henry Knox and other nationalists, whose views were focused at a gathering that "probably occurred" during the meeting of the Cincinnati that was held in Philadelphia as delegates waited for the convention to convene (vol. 3, p. 418).

Such arguments are guaranteed to generate a second storm of protest, as are the methods that support them. Countervailing evidence is repeatedly ignored or explained away. In passage after passage, contemporary language that might seem quite clear to any specialist in the history of the Confederation years is tortured into shapes that none will recognize. For all the claims to be recovering an eighteenth-century universe of discourse, the work shows little feeling for the period with which it deals. By ordinary standards of the discipline, it is outrageously bad history. By any test, it is a stunning feat of tendentious argumentation, a clever if exhausting brief for a position that Alexander Hamilton himself did not attempt to argue.

LANCE BANNING  
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JAMES HAW *et al.* *Stormy Patriot: The Life of Samuel Chase*. Baltimore: Maryland Historical Society. 1980. Pp. viii, 305. \$14.95.

Samuel Chase is one of those shadowy figures who appear in almost every American history textbook as the principal character of a single episode and then disappear. The four authors have rendered a distinct service in bringing life and substance to the man who is usually remembered only for the fact that his impeachment was the first real test of Congress's power to remove executive and judicial officers.

Chase grew up in Baltimore and then moved to Annapolis, where he embarked on his career as a lawyer. He began his political career as a radical agitator in the Stamp Act crisis of 1765 and was a leader of the Whig movement for independence in Maryland. During the War of Independence he served first in the Maryland legislature and then in the Continental Congress. By the end of the war Chase was a major figure in Maryland politics, although his reputation had been tainted by charges of war profiteering. His legal and political career reached a climax when he was appointed to the Supreme Court by President Washington in 1796. Justice Chase's stormy career was marked both by controversy and by solid contributions to American legal precedent. He undoubtedly had a brilliant legal mind, but his impulsive and tempestuous nature frequently led him into behavior that was both embarrassing and injudicious. Such episodes finally led to his impeachment.

Perhaps it is the multiple authorship that causes several flaws in this work. There is a continued effort to demonstrate consistency in Chase's political attitudes as he shifted from his early radicalism to Federalism. Yet this was not unusual or strange for any revolutionary generation. Once a revolution has succeeded and a new system has been established it is natural for those who have come to power to maintain their positions. That personification of Federalism, Alexander Hamilton, first called attention to himself as a flaming radical in the Whig movement in New York. Consistency was hardly the hallmark of late eighteenth-century American politicians.

There is also an unwarranted preoccupation with Chase's social status. The authors attribute Chase's failure to reach high social position to the fact that he never became a large property holder. It is true that wealth could open many doors in eighteenth-century America, but would those doors have been closed to a justice of the Supreme Court? One suspects that it was his bad temper, rough language, and crude manners that inhibited Chase's social ambitions.

Finally, aside from the excellent account of his impeachment, there are fewer than fifty pages devoted to Chase's career as Supreme Court justice. This seems unfortunate since this constituted his real contribution to American constitutional history. Nevertheless, this is a valuable addition to the biographical literature of the revolutionary generation and the authors are to be commended for bringing Samuel Chase to life.

JOHN PANCAKE  
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Tuscaloosa

FRANCIS N. STITES. *John Marshall: Defender of the Constitution*. (Library of American Biography.) Boston: Little, Brown. 1981. Pp. vi, 181. \$11.95.

Small books on American giants are too often disappointing to the serious reader. In a series of limited works such as the "Library of American Biography," authors sometimes lose sight of the imposed limitations and fail in an effort to write a comprehensive account of a person's life. The result in those cases is a book of passing interest to general readers but of no use to scholars. Fortunately, this new, very short biography of John Marshall is a significant departure from that occurrence and is a model of this genre.

Francis N. Stites has put together a surprisingly good account of Marshall's life and public career. This is the more surprising because Stites manages to get into his book a few interpretations and con-

ceptions that are not found in other, larger biographies of the famous chief justice. No full-scale biography has been written to replace Albert Beveridge's multi-volume work, now over half a century old, and yet much has been written about Marshall and his tenure on the Supreme Court of the United States.

It is evident that Stites has read everything available (although in the absence of footnotes one assumes he did not consult manuscript sources), and his is the first biography to make intelligent use of the first three volumes of *The Papers of John Marshall* that cover his career to 1798. If some sections of his book are too short (Marshall's service as Adams's last secretary of state receives seven words), Stites's restraint nevertheless leads to useful summaries of the important divisions of Marshall's life. In a nine-chapter book, five cover the Supreme Court period, dividing Marshall's tenure into such significant segments as the period of consolidation, followed by one of dominance, then accommodation, and finally decline after the election of Jackson. Written without the hyperbole of Beveridge and more recent Marshall biographers, these chapters would be suitable as lectures; they are well organized and thoughtful, although they would be best presented in an introductory course. They will not pass for serious examinations of constitutional law and Marshall's influence on that subject, nor do they appear intended for that purpose. The book is after all a biography and not a study of the law. It will be most useful as a summary of Marshall's life and his career as chief justice, containing little new information but offering some insight into a familiar topic by its impressive condensation of the large body of literature on the subject. Stites has thereby made a significant contribution to Marshall scholarship.

CHARLES T. CULLEN  
Princeton University

ROGER L. NICHOLS and PATRICK L. HALLEY. *Stephen Long and American Frontier Exploration*. East Brunswick, N.J.: University of Delaware Press. 1980. Pp. 276. \$19.50.

Although Stephen H. Long spent nearly fifty years in the service of the army engineers, he is remembered primarily for his activities during the period 1819–20. In 1819 he led an exploring expedition up the Missouri in a crude steamboat, the *Western Engineer*. When this "Yellowstone Expedition" (which never advanced beyond Council Bluffs) failed, Long was ordered west in an exploration of the region from the Central Plains to the Rockies. This scientific exploring expedition resulted in, among other accomplishments, the drawing of a map de-

scribing that region as the "Great American Desert."

Clearly there is more to be said about Major Long and his expeditions than these brief facts. Now two historians have focused on all his explorations; they cover the eight-year period 1816–24. Long's only importance historically is in these eight years when his attention was on western exploration. Roger L. Nichols and Patrick L. Halley narrate in detail Long's activities and analyze the scientific results of these expeditions.

Nichols and Halley also promote the thesis that Long has been treated unfairly by historians—that his expeditions were worthwhile and accomplished a great deal for the scientific community. The authors have done their research well and they make a good case for the scientific contributions. Their conclusions, embodied in chapter 8, "The Explorers Report to the Nation, 1821–1823," and chapter 11, "Stephen H. Long as an Explorer," point out especially the work done by two expedition members, Titian Peale and Thomas Say. Indeed, a substantial body of scientific information did result from the Long expeditions.

But what about Stephen Long as a leader and promoter of western exploration? Unfortunately he remains as he has always been portrayed, only perhaps worse. Long made serious judgment errors, quarreled with his colleagues, and often failed to carry out orders. This reviewer holds a lower estimate of the man after reading this book than he did before! Finally, the authors' suggestion that Long renewed army interest in scientific exploration is not convincing. Long simply enjoyed the work and endeavored successfully to get assigned to duties he had himself suggested.

The chief criticism of this book is its condescendence. Again and again the authors digress to inform the reader of the significance of some incident in the development of Long's character. Such statements as "These incidents raise questions about Stephen Long's capabilities as an explorer and leader" (p. 51) are unnecessary for an educated readership. Again, the authors fail to relate adequately Long's relationship with Colonel Henry Atkinson's Missouri expedition. There is considerable repetition throughout the book.

This study is a worthy contribution because it focuses on Long's expeditions exclusively, makes clear much that has heretofore been obscure, and does make the point that considerable scientific information resulted from his expeditions.

RICHARD A. BARTLETT  
Florida State University

LAWRENCE THOMAS LESICK. *The Lane Rebels: Evangelicalism and Antislavery in Antebellum America*. (Studies in



Evangelicalism, number 2.) Metuchen, N.J.: Scarecrow Press. 1980. Pp. ix, 278. \$15.00.

Early in 1834 students at the recently established Presbyterian Lane Theological Seminary in Cincinnati, under the presidency of Lyman Beecher, conducted a marathon discussion of the problem of slavery. After eighteen evenings devoted to a debate on the relative merits of abolitionism and colonizationism, most of the students became committed abolitionists, established an antislavery society, and undertook a program for educating and uplifting the free blacks of the city. The trustees and faculty forbade such activity in the future, and most of the students, "the Lane rebels," under the leadership of Theodore Dwight Weld, chose to withdraw from the seminary, many of them transferring to Oberlin College. In 1933 Gilbert Hobbs Barnes published an influential work, *The Antislavery Impulse, 1830-1844*, which included two chapters on this series of events. Now at last we have a full-length book on the subject.

*The Lane Rebels* originated as a doctoral dissertation in church history at Vanderbilt University. It is a very creditable piece of work, thoroughly researched, fully documented, well organized, competently written, and free of typographical errors. Lawrence Thomas Lesick's thesis is that in the Lane debates Charles G. Finney's "theology of revivalism was transposed to an antislavery setting" (p. 84). The so-called "debates" can be compared to a revival meeting through which the students came to see slavery as a sin. This book adds to but does not alter significantly Barnes's account of the episode.

After a short introduction Lesick discusses the life of Cincinnati in the 1830s and the founding of Lane Seminary. His treatment of the debates themselves does not add much that is new; detailed information on what was said in the discussion is simply not available. His chapter on the efforts of the trustees and the faculty to suppress student activism and the withdrawal of the students contains a good bit of fresh material. His major contribution is found in the chapter discussing the later activities of those who left the seminary. Seventy-five out of one hundred and three students withdrew. Of the fifty-four "rebels" for whom information is available, forty-two engaged in antislavery activities after 1834, many as agents of the American Anti-Slavery Society.

To this reviewer it seems that Lesick exaggerates the importance of evangelical Christianity in the abolition movement. Quakers, Unitarians, and free thinkers also labored in the cause. Many abolitionists were actually hostile to the evangelical churches, regarding them as "bulwarks of slavery."

Scholars interested in this subject should also examine the two chapters on "the Lane rebels" in the

fine new biography of Theodore Dwight Weld (*Passionate Liberator*), written by Robert H. Abzug (1980).

IRA V. BROWN

Pennsylvania State University

ALLAN PRED. *Urban Growth and City-Systems in the United States, 1840-1860*. (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1980. Pp. xv, 282. \$28.00.

The most recent of Allan Pred's monographs on the history of American *urban systems* is welcome. The idea of systems of cities is the foundation of Pred's approach: regional groups of urban places characterized by economic and demographic interdependence. A loose hierarchy of systems may be thought of, from a few places in a Massachusetts river valley, up through the whole of New England, the nation, and on to international systems. This approach changes the emphasis for students of urbanization from particular locales to a broad and long-term process. It probably also subsumes the notion of urbanization itself into the emergence of large-scale society characterized by large-scale organization—far transcending the limits of even the largest metropolitan (or "megapolitan") centers.

The period covered has its own importance; during these two decades urbanization proceeded more rapidly than in any comparable time before or since. The nation's essentially industrial future became evident, and the eastern half of its railroad map was practically established. All of this, clearly, is related to the emergence of larger urban systems, wider patterns of interdependence, and integration.

Pred discusses the extant urban history literature usefully, concluding that most of it contributes little to his own needs. Urban biographies and studies of city rivalries have too small a focus and seem to assume "closed systems." It should be remembered, though, that these studies are characteristically aimed at other problems than those which Pred attacks. Social structure, political culture, and government may or may not be partly explained by the study of urban systems; so far, they have yielded mostly to highly localized investigations.

There is a valuable discussion of the available statistics (pp. 22-33) and some of the pitfalls to snare the unwary. Most obvious—and even nowadays often overlooked—is the distortion caused by too much stress on percentage growth. This greatly exaggerates the growth of new places; some sophisticated correction leads Pred to conclude that "absolute urban growth" was highly concentrated in the already existing and sizable cities. He might have added another warning that deserves more analysis than it gets—the spotty value of the census figures

themselves. They are known to contain errors of large magnitudes; the art of counting is still under considerable suspicion, but it is certainly better today than it was when the 1840–60 censuses were taken.

Some readers will have trouble with Pred's agile math; they will profit from the book without trying to follow these operations. Some will find it too formal—but this is a built-in condition of model building. This book is not narrative history; it is a much-needed analysis of the stage on which the narratives have been enacted.

A. THEODORE BROWN  
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ARNOLD M. SHANKMAN. *The Pennsylvania Antiwar Movement, 1861–1865*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1980. Pp. 236. \$19.50.

This brief survey of political opposition to the Civil War in Pennsylvania asserts for the movement a significance comparable to that which it attained in the Midwest, the border states, and New York. The author, Arnold M. Shankman, begins his narrative with a description of a Democratic party in disarray in the aftermath of Republican gubernatorial and presidential victories in 1860, the secession crisis, and the failed search for compromise. With the outbreak of hostilities, "the overwhelming majority" (p. 64) of Democrats supported Lincoln's decision to suppress the rebellion by force, and thousands responded to his call for volunteers to do so.

By the fall of 1862, however, a significant antiwar faction had emerged within Democratic ranks, buoyed by dashed expectations of a quick Union victory and extended casualty lists, by "the ever-increasing centralization of power in Washington" (p. 82), and by opposition to Lincoln's announced intention to issue the Emancipation Proclamation. Antiwar Democrats, in turn, were divided. "Peace at any price" men—estimated as having never exceeded 10 percent of the state's voters—demanded an end to hostilities even if it meant permanent separation. Some urged that Pennsylvania join the Confederacy. A larger group, "peace Democrats" or "Copperheads," were more moderate and constituted "the loyal opposition to the Lincoln administration" (p. 15). Calling for an end to the fighting and reconciliation through negotiation, they gradually assumed control of the Democratic party under the slogan, "The Union as it was and the Constitution as it is." At the height of their influence in the fall of 1863, their candidate for governor, George W. Woodward, came within 15,325 votes of defeating incumbent Andrew G. Curtin in an election in which over 520,000 votes were cast. However,

Union victories in the field undermined the antiwar movement thereafter, and Lincoln's defeat of Democratic presidential candidate George B. McClellan in state and nation in November 1864 sounded the death knell for Copperheadism in Pennsylvania. By January 1865 "probably less than ten percent of the state's Democracy called for a non-military solution to the war" (p. 219). The enduring significance of the movement, the author concludes, is that despite personal vilification and worse, "Pennsylvania Copperheads stood up and reminded the nation that the Constitution applied both in time of war and in time of peace" (p. 219).

Shankman's account, although interesting, is undistinguished in style and largely descriptive in content. He makes little effort, quantitatively or otherwise, to analyze closely who the "peace Democrats" were or why they held their distinctive views. He is inclined, instead, to broad generalizations or to references to specific leaders only. Moreover, his treatment of the antiwar movement is uncritical, and he probes neither the motives of the participants nor the consequences of their conduct for the Lincoln administration or for the future of the Democratic party in Pennsylvania. Readers will be distracted by the placement of documentation at the end of each chapter, by the occasional absence of documentation of significant conclusions, and by scattered typographical errors—a few of which alter the author's presumed meaning. Nonetheless, as antiwar sentiment in Pennsylvania has not previously been subjected to extended scrutiny and its dimensions have been generally minimized, those interested in the subject will find this study both useful and corrective.

JOHN F. COLEMAN  
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DICKSON J. PRESTON. *Young Frederick Douglass: The Maryland Years*. Baltimore: Johns Hopkins University Press. 1980. Pp. xvii, 242. \$15.00.

Frederick Douglass clearly deserves his reputation as the major black figure of the nineteenth century. Spanning the final years of slavery and the first generation of freedom, his struggle for his personal freedom and for the liberation of his people is one of the great sagas of American history.

Douglass was the author of three separate autobiographical works published between 1845 and 1891 that cover his years as a slave, but this carefully researched work by journalist Dickson J. Preston offers a fresh new perspective. Digging deeply into archival materials, examining papers, and interviewing descendants of Talbot County slaveholding families, and steeping himself in a vast array of

genealogical lore, Preston provides an account of Frederick Douglass's twenty slave years detailed enough to satisfy the most meticulous seeker of facts. Here Dickson offers us biography at its best, bolstered by impressive bits of historical detective work, most notably the author's pinpointing of Frederick Douglass's time of birth, February 1818.

But this book strives to be more than mere biography. Boldly choosing to tread on some controversial historical ground, the author finds the footing difficult if not downright treacherous. Preston believes that Douglass's most widely read account of his slave years, his 1845 *Narrative of the Life of Frederick Douglass, An American Slave*, was an example of overblown antislavery rhetoric that badly exaggerated the evils of the institution's impact on the young Douglass. Rather than being a child of deprivation, Preston asserts, "Frederick was an exceptionally privileged one" who "for the seven most formative years of his youth . . . lived in virtual freedom" (p. xv). But while not frequently whipped, Douglass certainly saw all of slavery's evils firsthand and experienced many of them himself, in the process developing awarenesses that became for him the raw equivalent of many a bloody lashing.

If Douglass misspelled certain names, embellished facts, and mistook allegedly idle boasting by self-proclaimed slave-killers such as Thomas Lambdin for fact (an account unnecessarily detailed twice [pp. 74, 101]), he could not really be faulted. If Aunt Katy, the black cook on the Lloyd plantation, was "as cruel as any white overseer" (p. 53), the fact that she was black and often mistreated Frederick Douglass while protecting her own children says a great deal more about the divisive impact of slavery upon fellow blacks than it does about Douglass's supposedly exaggerated mistreatment at the hands of whites. In fact the author even admits that most of Douglass's perceptions usually were not wide of the mark.

According to Preston and contrary to Douglass, the Lloyds seldom sold slaves, "preferring to keep them in the family" (p. 43). Yet upon closer examination later on (pp. 57-58), the author appears to accept a more negative view, citing the selling off of twelve slaves by the Lloyds between 1825 and 1827. Small wonder if Frederick Douglass frequently chose to put slavery's worst face forward since "in the first fourteen years of Frederick's life," Preston admits, fifteen close relatives and "dozens of others whom he knew well disappeared without a trace into the deep South" (p. 76).

At the heart of the problem is not Preston's perception of Douglass. Rather it is his overly romanticized view of those landholders of Maryland's Eastern Shore who held Douglass slave. It was not their fault, according to Preston, that there was so much

evil. Rather it was slavery's absolute power that corrupted so absolutely. Without slavery, life would have been so different. "If they [Douglass and Thomas Auld, his last owner] had loved and trusted each other from the beginning, much of this could have been avoided," Preston believes, deeply troubled that Douglass unfairly made Auld "an object of scorn throughout the world" (p. 62).

Preston must then end his book with an unfortunately overblown account of Frederick Douglass's triumphal postbellum return to the scene of his boyhood years as a slave, to Maryland's fondly remembered Eastern Shore to meet with relatives of his old owners and even with octogenarian Thomas Auld himself. But Preston is himself taken in by it all, particularly by Douglass's oratorical skill at telling his white listeners exactly what they wanted to hear, a trick, as Preston knows, he had learned well as a slave. "I am an eastern shoresman," Douglass happily called out to his listeners, "with all that name implies . . . eastern shore corn and eastern shore pork gave me my muscle. I love Maryland and the eastern shore" (p. 3).

But Douglass had come to Maryland to convince its white citizenry to give blacks real equality, as Preston points out. It is wishful thinking when the author concludes that Douglass "hated nothing about Maryland except the fact of slavery, and now that emancipation had come, his love was complete" (pp. 162-63). The malice and hatred indeed were gone; Douglass was not one to harbor old grudges. But the nostalgic vision of an aging Frederick Douglass does not destroy or replace the cold and piercing young eyes of an unhappy slave angrily longing for freedom, who saw little that could be romanticized and much that was heartrendingly cruel.

DONALD M. JACOBS  
Northeastern University

DANIEL F. LITTLEFIELD, JR. *The Chickasaw Freedmen: A People Without a Country*. (Contributions in Afro-American and African Studies, number 54.) Westport, Conn.: Greenwood Press. 1980. Pp. xii, 248. \$25.00.

Critics of the National Endowment for the Humanities ought to be made aware of the work of Daniel F. Littlefield, Jr. He received a younger humanist award for the academic year 1973-74, during which he completed the research necessary to publish four monographs on the Afro-American experience among the Southeastern Indian tribes later resettled in Oklahoma. The most recent return on the NEH's investment is *The Chickasaw Freedmen*. Littlefield begins his study by briefly chronicling the develop-

ment and character of black slavery among the Chickasaws before and after their removal to the West, emphasizing that the institution of slavery was an influential channel for the transfer of white culture to the Indians. The tribe reluctantly emancipated its 2,000 slaves after the Civil War and, with the Choctaws, agreed in 1866 either to adopt their freedmen within two years or to forfeit funds due the two tribes for lands appropriated by the United States. In the latter case the federal government would use the money to resettle the blacks elsewhere in Indian Territory. Although the Choctaws eventually did so, the Chickasaws feared political domination by their freedmen and steadfastly refused to give them citizenship rights. At the same time, the United States declined to remove the blacks according to the requirements of the 1866 treaty. Considered intruders by the Indians and ignored by the federal government, the freedmen became, according to the author, "a people without a country."

The bulk of the book is devoted to describing the anomalous condition of the freedmen from 1868 through 1902, when the Chickasaws were forced to acknowledge their vested interests. Using source material creatively, Littlefield characterizes the economic and social life of the ex-slaves, their struggle to obtain educational facilities, the failure of proposals to remove them to another location because of the government's wish not to impede white settlement, their efforts to be enrolled by the Dawes Commission after 1893, and their eventual allotment of 40 acres each of the Chickasaw estate after 1903. The 4,662 freedmen, however, never won legal recognition of their claims for citizenship within the tribe, and the government was required to reimburse the Indians for those acres they did receive. Moreover, many were left off the tribal rolls even though they had a greater percentage of Chickasaw blood than many people who were included.

The strengths of this book are many. It addresses a dimension of black and Indian history that has not previously been considered, and it is based almost entirely on primary source materials. Indeed, not since Angie Debo has anyone mined the records of the Dawes Commission quite so thoroughly as Littlefield. But the study also has problems. Most significantly, the author provides very little analysis in his narrative and seldom relates events to the broader context of freedman or Indian history. For example, he accepts uncritically the many petitions generated by the blacks, never assuming that they may have been solicited by professional claims agents or whites who hoped to weaken tribal control of the land. Also, the detail and organization of the narrative make for tedious and sometimes repetitive reading. Finally, endnote style is untraditional

and the typographical format is distracting. These things aside, Littlefield has made a pioneering contribution and declared thereby yet another dividend on a modest NEH investment.

W. DAVID BAIRD

Oklahoma State University

THEDA PERDUE. *Nations Remembered: An Oral History of the Five Civilized Tribes, 1865-1907*. (Contributions in Ethnic Studies, number 1.) Westport, Conn.: Greenwood Press. 1980. Pp. xxiv, 221. \$23.95.

American historians are generally aware of the WPA's services to our profession during the 1930s, particularly that agency's publication of the "American Guide Series." Less well known are other endeavors by the WPA, such as oral interviews of old-timers in a number of states. In Oklahoma during the depression era there still existed a number of Indians and white pioneers who had witnessed the change in the land's status from Indian Territory to statehood and whose testimony about the events would be important to later generations. Personal recollections were collected under the direction of Grant Foreman, the state's leading historian, and gathered as the "Indian-Pioneer History." Today the results of those interviews exist as typescripts bound in some 112 volumes at the University of Oklahoma and Oklahoma Historical Society.

Theda Perdue has examined these volumes and selected comments from Indian witnesses and descendants of the Five Civilized Tribes, which she has grouped under topics such as law and disorder, subsistence, entertainment, traditions, religion, and education. This is a very selective edition, and the author points out that she omitted "mere chronicles of political events and controversies" (p. xxiii). She also worked under guidelines of the holding institutions that allowed no interview to be used in its entirety and no narrator to be identified. I was not disturbed by these strictures and do not believe they hampered the quality of the work.

Persons interested in Oklahoma's formative period have used these volumes over the years, but always with the caution that they should be checked carefully against more reliable documentary evidence. In her annotation Perdue has done this—correcting historical inaccuracies and identifying individuals who are named by the narrator. More scholarship could have been brought to bear on this area, however, since well-known persons were given a fuller citation than necessary while others received but slight attention in the footnotes. If the latter individuals were so obscure as to be unidentifiable, then the author should have made this clear. There is also no information given about the qualifications



of the narrator or the skill of the interviewer. Such data could have been given without revealing the names of the persons involved. In spite of these criticisms, readers should welcome the first publication from this valuable collection.

GARY E. MOULTON

*Journals of the Lewis and Clark Expedition*  
University of Nebraska,  
Lincoln

PEGGY A. RABKIN. *Fathers to Daughters: The Legal Foundations of Female Emancipation*. (Contributions in Legal Studies, number 11.) Westport, Conn.: Greenwood Press. 1980. Pp. viii, 214. \$25.00.

In their search for the origins of the nineteenth-century women's movement, historians tend to rely on one primary source, the six-volume *History of Woman Suffrage* edited by Elizabeth Cady Stanton, Susan B. Anthony, and Matilda Joslyn Gage. Accepting these participants' version of the beginning of the ferment for women's rights, most scholars have found the origins of the nineteenth-century feminist movement in the effort to abolish slavery.

As a result of her study of the development of legislation concerning married women's property rights in New York State, Peggy A. Rabkin concludes that the "movement to reform the law of property through legislation that would sweep feudal relations and concepts out of the law of real property initiated the first American women's rights movement" (p. 12).

After the American Revolution, Rabkin argues, laws were modified in New York State in an effort to make land an item of commerce and to codify and democratize the law. Law reform and economic factors, not human rights concerns, motivated the legislators to pass an act in 1848 to protect married women's property from their husbands, but more importantly, from their husbands' creditors.

The first part of the book deals with the intellectual and jurisprudential origins of the Married Women's Property Acts, which replaced feudal concepts of real property law—common law and equity—with laws established by elected representatives of the people. These acts replaced the equity or trust system of protecting married women's property, which relied on judges' interpretations and left a great deal of uncertainty about the security of the estates that fathers desired to descend to their daughters and grandchildren rather than pass, as Elizabeth Cady Stanton said, "into the hands of dissipated, thriftless sons-in-law" (p. 154).

In the second part of the book there is a review of the interaction between statutes and cases in New York State, and Rabkin finds that the Married Women's Property Acts resulted in few husbands and wives going before the courts as adverse parties;

most suits involving these acts were brought by or against creditors or employers.

Rabkin's position that there is a need to study the development of law to understand the development of the women's movement was suggested in earlier studies, including those of Mary Beard and Keith Melder, but no one before concentrated in such detail on the development of the law. As Rabkin says, the focus needs to be on the impact of legislation on feminism rather than the impact of feminism on legislation; "the enlightened legislative reform, which was centered in New York State after the American Revolution, triggered the demand for female suffrage and not vice versa as prior scholarship suggested" (p. 12). In other words, once married women could legally hold property, the next logical demand was for suffrage to provide for the protection of that property.

The point that Rabkin makes in her concise, closely argued study is that more than one factor contributed to the rise of the women's movement in the mid-nineteenth century. Further, she explains women's rights under common law and suggests that political and economic motives, not feminist arguments of human rights, caused lawmakers, in this and other cases, to redefine women's legal status. Some questions that follow from the study are: What impact did the Dutch tradition in New York have on the modification of the common law? Is the development of women's property rights parallel in other states?

Although the author's conclusions are important for students of feminism, the book is a study of the law and, as such, is an important addition to "Contributions in Legal Studies."

BEVERLY BEETON

Governors State University

KLAUS J. HANSEN. *Mormonism and the American Experience*. (Chicago History of American Religion.) Chicago: University of Chicago Press. 1981. Pp. xviii, 257. \$15.00.

Klaus J. Hansen's *Mormonism and the American Experience* is an interesting and for the most part well-integrated essay in the "Chicago History of American Religion" series edited by Martin E. Marty. Hansen has drawn together much of the significant work of numerous scholars engaged in the writing of the "new Mormon history," together with many recent interpretations of Jacksonian America, to shape a volume that is, perhaps, the best single synthesis that we have. Hansen argues that Mormonism has undergone enormous change during its 150-year existence, moving from a pre-industrial society opposed to the "privatization" of American life in economics, family, religion, and politics to a modern

religion, in Robert Bellah's terms, that has privatized values, giving up its attempt at broad and rigorous social control to allow for individual interpretation of Mormon doctrine, private enterprise, and political pluralism. Just recently, Hansen indicates, there is a new attempt at external control in the areas of sex and life style. All of which seems to suggest that Hansen believes that the Mormons have generally followed national trends, only with a considerable cultural lag.

Although Hansen relies heavily upon Mark Leone's recent book, *The Roots of Modern Mormonism*, to describe twentieth-century changes in Mormon thought, moving from outer direction to inner direction, Hansen actually takes issue with Leone to a considerable extent by demonstrating that the Mormons have a fairly explicit and consistent belief system. Hansen says that Leone has overstated "the case for the fluidity of the Mormon belief system" (p. 247) and demonstrates it rather well by reviewing Mormon doctrine against the background of American religious thought. In one of his most discerning chapters, Hansen argues that the Mormon belief in the continuity of individual identity and experience in the next life came as a reaction to an almost obsessive concern with death in the early nineteenth century.

If there are weaknesses in Hansen's largely social and intellectual history of Mormonism, it comes from his failure (p. 106) to keep abreast of some of the latest articles in the field, especially those of D. Michael Quinn, who has shown, contrary to Hansen, that Joseph Smith made several attempts to name a successor. The very recent discovery of the blessing of Joseph III to succeed his father only adds weight to Quinn's thesis. More importantly, Hansen (p. 142) ignores Quinn's recent piece that takes issue with him as to the importance of the Council of Fifty after 1845. Quinn uses sources overlooked by Hansen to question whether Brigham Young had need for the council once the territorial government was established and all the other churchly tribunals were operative. My own reading of the diary of Anthon H. Lund, one of the Church Presidency after 1900, suggests that although the council was of no import, the political kingdom was still very much alive in the sense that important political decisions were made at the church office.

Also, I am not sure that Mormonism was a response to industrialism as Leone affirmed and Hansen seems to accept. Leone offered no evidence for this, simply stated it. We need to know a lot more from a demographic viewpoint about Palmyra, Manchester, Colesville, and other Mormon starting places before we can be sure what changes were taking place, if any, and what the socioeconomic status of Mormon converts was as compared to others who did not join the movement. Hansen's disposition to

call the early Mormons pre-industrial (p. 125), and then to say that Mormonism was, afterward, an affirmation of the work ethic of an industrial society (p. 111), requires clarification and demonstration in terms of the kind of people the Mormon message appealed to at its various stages of transformation. The fault is not Hansen's that adequate demographic work has not been done.

While historians have something left to do on the social implications of the Mormon kingdom, it seems to me that Hansen's synthesis marks a culmination of a thirty-year quest to define what the sociopolitical aims of the Mormons were, and his chapter on the Mormon way of death points toward a new direction in assessing the inner dynamics of the movement. In the face of such drastic change, over a period of time relatively short as religions go, how did the individual Mormon adjust his thinking? What were the attitudes and institutions that did not change, or only changed subtly so that the Mormon was sheltered from the full impact of the revolutionary changes that went on within and without the church? Until historians produce more biography with these questions in mind we are left without an answer.

MARVIN S. HILL

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ROBERT A. SILVERMAN. *Law and Urban Growth: Civil Litigation in the Boston Trial Courts, 1880-1900*. Princeton: Princeton University Press. 1981. Pp. xiv, 217. \$16.50.

This is a study that operates at the borderline of urban and legal history. Robert A. Silverman studies civil litigation in the Boston trial courts between 1880 and 1900 and concludes that "novel urban habits and practices" during those years "demanded legal controls over the hazards created by crowded living" (p. 3). One of his "underlying assumption[s]" is that the business of trial courts "mirrors the social and economic life of which they are instruments" (p. 6).

Within his assumed framework Silverman contributes some useful, if not startling, information. Most litigants were middle-aged, middle-income, white, male, and of native parentage. Lawyers were "overwhelmingly white and Yankee" (p. 31). The more liberal the credit terms of a business, the more likely that business was to resort to litigation to collect its debts: retail clothiers undertook commercial credit suits much more than retail liquor dealers. Most real estate suits concerned themselves with landlord-tenant issues. "The less efficient the trial courts became, the more often they were bypassed" as mechanisms for resolving disputes (p. 144).

Silverman's most interesting finding, given recent scholarly debate about the origin and growth of

nineteenth-century tort law, is that "a monstrously large and perplexing" growth in negligence cases during the period was paralleled by a dramatic rise in streetcar accidents. One might conclude, as have some commentators, that the development of negligence law between 1880 and 1900 was simply a response to the increased dangers of urban life. But Silverman also finds that "the number of [tort] lawsuits that were not directly connected to technological change" also increased substantially, from approximately fifty in 1880 to over six hundred in 1900. He concludes that in late-nineteenth-century Boston litigation "became an increasingly popular response to injury," and Bostonians became more litigious "by choice as well as out of necessity" (p. 112).

Silverman's findings thus seem to support the following explanatory theory of the growth of late-nineteenth-century tort law. A combination of increased urban accidents and public perceptions that tort doctrine was capable of providing some relief from those accidents generated increased tort litigation. Tort cases would not have grown "monstrously large" without the accidents, but they also would not have grown without a public sense that tort doctrine was at least in some measure responsive to the problem of fortuitous injuries. Such an explanation revives a familiar dilemma of historical causation: does discernible social change occur because events "happen" or because persons perceive the existence of certain events as significant enough to warrant public intervention? It is one thing to be injured by the apparent carelessness of a stranger and to want redress; it is another thing to regard one's injury as enough of a social problem that a public mechanism like the courts might provide that redress. Both factors were central to a decision to litigate a tort claim in late-nineteenth-century Boston.

Thus Silverman's findings neither decisively intervene in a current debate nor dramatically revise existing assumptions about the kinds of persons who engaged in civil litigation and their reasons for that decision. *Law and Urban Growth* is nonetheless a solid, if modest, contribution. It demonstrates that the function of trial courts in late-nineteenth-century Boston was to address "those disputes . . . where only the weight of the state could bring a satisfactory ending" (p. 142).

G. EDWARD WHITE  
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DOLORES GREENBERG. *Financiers and Railroads, 1869-1889: A Study of Morton, Bliss, and Company*. Newark: University of Delaware Press. 1980. Pp. 286. \$22.50.

This book by Dolores Greenberg covers the business activities of a major private bank as it served the

needs of a growing rail industry in the 1870s and 1880s. When Morton, Bliss, and Company was formed in 1869 in New York City so many new bankers and brokers were appearing that a common saying of the day was that "to be a banker all one needed was to dress like one" (p. 27). Levi Parsons Morton, the son of a Vermont Congregational minister, had served long years as a New Hampshire storekeeper and later the proprietor of a wholesale and importing business in Boston and New York before he entered banking. George Bliss, Morton's banking partner and a former Connecticut dry-goods merchant, brought experience in international credits and an interest in railroading to the partnership. Morton's English partner, Sir John Rose, had resigned as the Canadian minister of finance to become the senior partner in the London firm, Morton, Rose, and Company. Morton's banking house, along with Drexel, Morgan, and Company, prospered in the years following the decline of Jay Cooke's banking firm.

During the two decades between 1869 and 1889 the pace of American railroad construction varied widely. In the early post-Civil War years the rate of new construction was rapid, and from 1867 to 1873 the railroad mileage in the nation increased from thirty-nine thousand to seventy thousand miles. As a result the total rail investment (capital stock and bonded debt) more than tripled in these years, and the bankers of the nation were kept busy meeting the financial needs of railroads. Morton, Bliss, and Company participated fully in providing the new money following a policy that George Bliss called a "Young America Spirit" (p. 51). The marked decline in construction during the depression of the mid-1870s found both Morton and Bliss more cautious as they pursued diverse tactics to salvage their own and their clients' investments.

The late seventies saw a returning optimism in railroading, and between 1878 and 1889 both the nation's rail mileage and rail investment roughly doubled. Again the Morton firm was very active in the growing rail investment opportunities. Before long the expansion was so rapid that the possibility of inadequate cash reserves replaced the earlier concern over inadequate business. During the eighties George Bliss became the more active partner as Levi Morton became more and more involved in politics. Morton was elected to Congress in 1878, received an appointment as minister to France in 1881, and was elected vice president in 1888. As Bliss sought the new business he found himself often competing with the "young" J. P. Morgan, a man he found to be "arrogant, impulsive, and dyspeptic" (p. 167).

The author unfolds a complex story of a private banking firm as it faced numerous financial and legal railroad problems. The involved account is strictly a business history, and few nonbusiness as-

pects in the lives of Bliss or Morton are included. Greenberg bases her work on a wide range of original sources with a major reliance upon the papers of George Bliss. Contemporary maps of several railroads served by the Morton firm accompany the text. An appendix of railroad-banker affiliations reveals that the Morton firm held directorships in a dozen railroads in 1873. This volume is an important review of the relationship of postbellum private bankers as they served an expanding railroad industry.

JOHN F. STOVER  
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STEPHEN J. PYNE. *Grove Karl Gilbert: A Great Engine of Research*. Austin: University of Texas Press. 1980. Pp. xiv, 306. \$20.00.

Grove Karl Gilbert (1843–1918) was one of the pre-eminent geologists of his generation. Many contemporaries regarded his work and intellect as the outstanding exemplar of study of the earth sciences. Gilbert, like the United States Geological Survey that defined his life, was a veritable “engine of research.” Stephen J. Pyne describes and analyzes this creative engagement, focusing on Gilbert’s singular efforts to lay the bases for the new science of geomorphology by assessing his subject’s path-marking studies of the Henry Mountains and Lake Bonneville. In establishing dynamic concepts of mountain formation, revealing the history of the Great Basin, and conducting important experiments in physical geology, Gilbert is portrayed at once in congruence with the institutional style of Geological Survey research and also as a scientist in notable contrast to the formalism, traditional descriptive interests, and paleontological research that marked the spirit of his scientific age. Gilbert’s skills bore a modern stamp of precision and prescience, highlighting his abiding interest in associating geology with mathematics, mechanics, physics, and engineering. In an age of great dinosaur hunts and grand museums to house such marvels, Gilbert stood in sharp contrast to the fame and fortune of such men as Othniel Charles Marsh of Yale or Henry Fairfield Osborn of the American Museum of Natural History.

Pyne’s thorough research also illuminates the significance of such figures as Samuel Emmons, George F. Becker, and Thomas C. Chamberlain, providing enlightening group biographies of individuals who, like Gilbert, too rarely receive appreciation in general assessments of national intellectual achievement. Gilbert’s admiration from contemporaries, however, provided Pyne with the challenge and the necessity of comparing the idealism of the past with the reality of modern biographical appraisal. The result is admirably successful in presenting a finely drawn picture of the emerging sciences of geomorphology and geophysics, accom-

plished by examining the structure of Gilbert’s achievements. Ultimately, Pyne concludes with the same overarching admiration that the men of Gilbert’s time shared about this distinctive individual. In so doing, he rescues his subject from current obscurity and gives flesh and substance to the sentiment of the past. Gilbert is thus appraised as a “superb” scientist, whose concepts speak “in the present sense,” in language that merges the physical and natural history of life. In this definitive biography Gilbert is defined as “a real man of genius in a discipline known more for its adventurers than its intellectuals.”

Too often, perhaps, Pyne, like other historians of the scientific drama of the American West and its protagonists, feels compelled to describe Gilbert’s achievements in words and phrases as romantic as the very environment that engaged him, offering repeated analogies between the man and the earth he studied. But that world needs celebrating in these times of moribund federal attitudes toward the support of intellectual activity. In encapsulating the life in science of an exemplary figure identified with the great achievements of a past too little appreciated, Pyne has written a book that, like his subject’s work, will endure and edify.

EDWARD LURIE  
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ROBERT W. CHERNY. *Populism, Progressivism, and the Transformation of Nebraska Politics, 1885–1915*. Lincoln: University of Nebraska Press, for the Center for Great Plains Studies, University of Nebraska, Lincoln. 1981. Pp. xviii, 227. \$17.50.

Robert W. Cherny examines “the political system of Nebraska during and immediately before the Populist and progressive period” (p. xv). Starting with the almost one-party Republican Nebraska of the 1880s, he takes the reader through the upheavals of the Populist years—a time of realignment—to Nebraska’s “first progressive era” (1906–12) and its aftermath. The book rests considerably on analysis of voting and voters, and how ethnocultural, rural-urban, and economic variables related to voting, as shown by stepwise multiple regression and cluster analysis.

The book consists of four pairs of chapters and a neat, intelligent conclusion. Each pair includes a narrative of electoral events and an analysis of voting behavior, leadership, and the political system of the moment. The narratives are very spare and the analyses elaborate, but the combination satisfies an organizational problem. The first pair (chaps. 1 and 2) deals with the 1880s, the second with the Populist years of 1890–96, the third with the transition of 1896–1904, and the fourth with Progressivism, 1905–15.

Cherny’s book touches on many issues in Populist



and Progressive historiography and should be read by anyone with serious interests in those areas. I can mention only a few. (1) Ethnocultural issues were highly salient for Democrats, economic issues for Populists, so much so that these two "outsider" parties were almost single-issue parties. This finding cuts across the pietist-ritualist model in a new way. Fusion between Populists and Democrats in Nebraska, effected from 1894 on, meant no loss of principle for either because they were interested in different things. (2) The 1896 election does not emerge as the beginning of the fourth electoral system in Nebraska. Instead of Republican dominance it ushered in almost a decade of close balance. (3) Populists were not pietists (or ritualists either). (4) Nebraska Populism was not a "shadow movement" but an intense political response to economic distress, which from its beginnings in 1889 stressed currency expansion as a basic reform. (5) The similarities between Populism in Nebraska and in Kansas appear clearer than before, for example, legislative inexperience, opposition to the A.P.A., many foreign-born and foreign-stock voters and leaders, attempted sabotage by a "Middle-of-the-Road" faction underwritten by the Republican state leadership, and very similar profiles of Populists regarding age, education, occupation, previous political experience, birthplace, and other characteristics. (6) As for the relation between Populism and Progressivism, Cherny finds that in voting behavior the pattern was mixed, in leadership they were very different, and in policy "they were also more different than similar" (p. 158): Populists wanted cooperation, currency reform, and redistribution of wealth, while Progressives, especially Republicans, wanted only regulation. Republican platforms, not Populist ones, contained planks about sterilization, divorce, liquor, and other cultural-moral issues. (7) The Republican shift from Old Guard to Progressive was complex and incomplete but involved a generational change, while the party remained the party of Anglo-conformity and moralism. Democrats became economic regulators but remained anti-prohibitionist. Here the ethnocultural model extends well past the more usual period (1888-92) into the twentieth century.

These hardly exhaust the contributions of Cherny's enlightening book.

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DONALD F. TINGLEY. *The Structuring of a State: The History of Illinois, 1899 to 1928*. (Sesquicentennial History of Illinois, number 5.) Urbana: University of Illinois Press, for the Illinois Sesquicentennial Commission and the Illinois State Historical Society. 1980. Pp. viii, 431. \$20.00.

During the 1960s several states commissioned official multivolume histories to provide up-to-date syntheses of the most recent scholarship. The problem came when the scholarly bases for these histories took a radically new direction even as the authors were writing the official histories, a direction that threatened to undermine their value for the profession. States are, after all, political subdivisions, and the thrust of scholarship in the 1970s was to shun descriptive political history in favor of social and cultural analysis and to turn the focus from political subdivisions like states toward communities and social groups. Even so, states are a good place to test larger generalizations about the impact of national events like the New Deal or World War I precisely because they are political subdivisions.

Donald F. Tingley's book, *The Structuring of a State*, accomplishes none of these goals. The second of three official sesquicentennial volumes on Illinois history from 1865 to 1968, it does not explore whether there was anything unique about Illinois. Indifferent to the interpretive questions that have interested historians of the period from 1899 to 1928, Tingley prefers to relate details. The two chapters on politics, for example, simply describe the different candidates without exploring alignments or underlying cultural tensions. In the 1920s "conservatism, apathy, and corruption had triumphed in Illinois" (p. 371), but we do not learn why except that voters had "tired of progressivism and Wilson's idealism" (p. 356). Indifferent to interpretation and criteria, he seems unaware of the contradiction in his discussion of the position of labor between the assertion that "retail employees suffered the most" (p. 73) and the later declaration that "garment industries . . . treated their employees . . . the worst both in terms of pay and working conditions" (p. 77). He writes paragraphs on each Chicago writer, conductor, and journalist rather than trying to explain what it was about Chicago that led H. L. Mencken, in a quote Tingley uses, to observe that most original and American writers had "some sort of connection with the Abattoir by the Lake" (p. 138). Popular culture emerged in the 1920s because "it was necessary to create new outlets to fill the idle time of the less affluent and the less well-educated" (p. 320).

There are a few small errors (such as identifying Hiram Johnson as a senator five years before he went to Washington), but generally the details are accurate. The chapter on World War I demonstrates the lack of enthusiasm for the war in Illinois. This was the only surprise in this otherwise predictable compendium of information.

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LEWIS L. GOULD. *The Presidency of William McKinley*. (American Presidency Series.) Lawrence: Regents Press of Kansas. 1980. Pp. xi, 294. \$15.00.

The historical reputation of William McKinley, one of the few twice-elected presidents of the United States, may have suffered from some bad luck. Possibly more people remember Vachel Lindsay's "Bryan, Bryan" than they do the victor in the great election of 1896. Then, his official biography is tedious and ordinary. Lastly, McKinley was succeeded on his assassination by one of the best-known personalities, activists, and showmen in the White House, Theodore Roosevelt. As a result, McKinley's memory faded quickly.

McKinley's reputation suffered in his lifetime and suffered until recently from the beliefs that he was weak, indecisive, pliable, and, most of all, that he was a puppet of Mark Hanna who in turn was a mouthpiece of big business.

Yet there were important contemporaries who had a different view of McKinley. People as different as Robert M. La Follette, John Hay, Joseph B. Foraker, Henry Adams, and Charles Dawes thought that McKinley had rare tact as a manager of men, that he usually accomplished what he sought, and that he "achieved very remarkable results." From veteran senators such as Shelby M. Cullom and George F. Hoar came high praise of McKinley's leadership of Congress. Because McKinley was not so pugnacious nor theatrical as some other presidents, he was often regarded as colorless, weak, and ineffective.

In recent years McKinley has been portrayed by writers as a strong president and as the first president in a long time (some say the first one since Jefferson) to provide Congress with active and effective leadership. And he is no longer seen as the weak president pushed into a war he did not want and as the "stooge" of Hanna.

Lewis L. Gould of the University of Texas in this first-rate and admirable new study of McKinley's presidency continues and expands the favorable reappraisals of McKinley begun in the 1950s and 1960s. Gould goes beyond the earlier ones when he argues persuasively and with evidence from a variety of sources and examples that McKinley was our first modern president. He emphasizes this point throughout his excellent book and, in fact, concludes that by 1901, "the nation had . . . a president whose manner and bearing anticipated the imperial executives of six decades later" (p. viii).

According to Gould, McKinley "laid the foundation for the modern presidency" by his "courageous" and "principled" presidential leadership during the coming of the Spanish-American War (he was not pushed into it), by the way he conducted and oversaw the war itself, and by the man-

ner in which he made peace with Spain, acquired the Philippines, and gained approval of the Treaty of Paris (1898) in the Senate. In all this he became "an imperial tutor" to the American people and "transformed the presidential office from its late-nineteenth century weakness into a recognizable prototype of its present-day form" (pp. 121, 152).

In addition, says Gould, McKinley in "unprecedented activities" beyond the war such as administering Cuba, putting down an insurrection in the Philippines, and sending American troops to China during the Boxer Rebellion in 1900 without congressional authorization, relied on the war power and his constitutional function as commander-in-chief—just as presidents since World War II have done—to justify American intervention abroad.

These are but some of the stimulating points Gould makes. He has written a splendid book about McKinley's presidency that supersedes the other accounts of this administration in analysis and perception. Gould's book has the virtues of being tightly and clearly written, deeply researched in both American and European sources, and balanced on the topics he has examined. While he does not make McKinley a great president, he does make him a figure of higher importance and interest than he has usually been. And he has also made him a more attractive figure. One might disagree with Gould here and there in the book, but overall it is a fine performance and will, I believe, be the standard work on McKinley's presidency for years.

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JAMES L. ABRAHAMSON. *America Arms for a New Century: The Making of a Great Military Power*. New York: Free Press. 1981. Pp. xv, 253. \$17.95.

Stimulated by the general interest among American historians about the origins of Progressivism, analysts of American military policy and institutions have written a substantial number of biographies and special studies about the most dramatic era of military reform in the United States before the Cold War. *America Arms for a New Century* synthesizes this work and should replace Peter Karsten's "Armed Progressives: The Military Reorganizes for the American Century," in Jerry Israel, editor, *Building the Organizational Society* (1972), as the most useful single analysis thus far of military Progressivism. James L. Abrahamson's study is also superior to the section on "neo-conservative" military reform in Samuel P. Huntington's overrated *The Soldier and the State* (1957). Although the book holds no surprises for military history specialists, it is an excellent introduction to the core issues of military reform and should be read by all experts on the Progressive period.

Abrahamson looks at military reform from the perspective of the line officers of the army and navy and concludes that their views on the relationship of diplomacy and military policy fell into the middle ground between isolationism and imperialism. The intraservice debate on missions and organization is the central concern of *America Arms*, but the author also attempts to connect military thought with the basic themes of Progressivism and demonstrates the class and ideological ties between the political-business elite and military officers. He does not accept the Karsten-C. Wright Mills thesis that military-managerial ties could dominate policy making, for the business and military worlds remained hostile to one another and internally divided as well. This conclusion must, however, be viewed as tentative since Abrahamson does not deal in any great degree with technological-logistical interrelationships between the services and American industry, generalizing instead from a single case study, Dean C. Allard's work on the navy and the steel industry.

*America Arms* argues that military officers developed their views about the need and extent of reform largely from external stimuli, principally changes in military technology and the expansionist urges of American foreign policy. A change of function for the regular services provided the central thrust: to prepare for the increased chance of conflict with another industrialized power the army and navy would have to abandon their peacetime constabulary functions and focus instead upon preparedness-related weapons development and unit training. As efficiency-conservative Progressives, military reformers abhorred the bumbling that characterized the campaign of 1898 and attempted to institutionalize rational planning and officer professionalization. In Abrahamson's view, officer self-interest and the thoughtless imitation of industrial-corporate organizations and foreign military institutions were not critical influences upon the reform impulse. This conclusion bears further thought, but Abrahamson makes a case that the obvious causes of reform may also be the most important ones. Perhaps that is the book's real novelty.

In his effort to be comprehensive, Abrahamson touches upon several attractive issues that he then does not investigate. Part of the problem is his definition of his subject and his relative dependence upon printed sources. For example, he might have linked the changes in military thought to the all-service policy of large unit maneuvers and the creation of contingency plans. A discussion of the cruise of the Great White Fleet, the Navy-Marine Corps advanced base exercise of 1914, and the Army-National Guard maneuvers in New England in 1912 would have provided dramatic evidence of the reorientation of American defense. Another issue that receives only cursory attention is the role of civilian

officials in military reform. Although Abrahamson links military and civilian thought, he does not tie reform writing with reform action in the executive branch and Congress.

*America Arms* is an admirable effort to place reformist military thought within the Progressive movement, and the book is an intelligent, well-written introduction to a watershed in American military history. Since the United States did not arm much before World War I (with the exception of the battleline of the fleet) or exercise appreciable global influence through military forces-in-being, the book's title is misleading, but Abrahamson's study should stimulate resurgent debate on military Progressivism. It provides the framework for additional analysis of a significant issue in the history of American military policy.

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PAOLO E. COLETTA. *French Ensor Chadwick: Scholarly Warrior*. Lanham, Md.: University Press of America. 1980. Pp. vii, 256. Cloth \$18.75, paper \$10.75.

In 1972 Peter Karsten's *Naval Aristocracy* introduced a provocative interpretation of late nineteenth-century graduates of the U.S. Naval Academy. According to Karsten, these "Annapolites" comprised an inbred, career-driven, reactionary Anglo-Saxon clique preoccupied with navalism and imperial expansion. During the past decade this controversial stereotype has drawn some criticism, but few detailed examinations of what Karsten refers to as "Mahan's messmates." Recently, however, Paolo E. Coletta of the U.S. Naval Academy has published biographies of three turn-of-the-century Annapolites, Bradley Fiske, Bowman H. McCalla, and now French Ensor Chadwick.

Chadwick seems the ideal figure with which to examine both Karsten's image of academy graduates and the larger organizational, technological, and diplomatic framework surrounding development of the new American navy between 1881 and 1914. Chadwick was a loyal careerist, who, while never upsetting the status quo in the naval establishment, actively promoted moderate naval reforms and expansion. Stationed in London between 1882-89 as the first permanent U.S. naval attaché, he gathered volumes of data to assist the navy in its transformation from a fleet of ancient wooden sailing ships into a modern force of steel steamers. Later, as chief of the equipment bureau, he improved further the mechanical apparatus of sea power, also aiding in various programs to improve personnel, naval yards, and bureaucratic organization. He was a sailing and fighting officer as well, commanding a gunboat in the navy's first squadron of evolution, a cruiser in Cuban waters during the

Spanish-American War, and a squadron off the North African coast during the Pedicaris affair of 1904.

Coletta follows Chadwick's life and naval environment in great detail. What emerges is the story of an average senior officer pursuing a normal naval career. He was not an activist insurgent like contemporary William S. Sims, a daring commander like George Dewey, or an innovative inventor like Bradley Fiske. Although a competent historian, his work was overshadowed by Mahan's syntheses. In fact, only in his pro-German sympathies during World War I did Chadwick depart from the stereotype cast by Karsten. The very predictability of Chadwick's personality, ideas, and objectives, however, provides Coletta with a perfect subject to test Karsten's entire thesis. Unfortunately, unlike his earlier study of McCalla, in this biography of Chadwick Coletta never exploits the opportunity. Nevertheless this is a competent naval biography and a contribution to the literature of the new American navy.

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GEORGE F. PEARCE. *The U.S. Navy in Pensacola: From Sailing Ships to Naval Aviation (1825-1930)*. (A University of West Florida Book.) Pensacola: University Presses of Florida. 1980. Pp. vii, 207. \$17.00.

George F. Pearce provides a useful survey of the U.S. Navy's presence in Pensacola over a period of 105 years. The navy yard established at Pensacola in 1825 initially served as a base for the American West India Squadron. Early efforts additionally to establish a major shipbuilding and repair center were impeded by yellow fever epidemics, labor shortages, poor overland transportation connections, and possibly by the limited political influence of territorial Florida. The need for better industrial capabilities at the station became evident during the Mexican War and led to improvements that allowed Pensacola to complete its first warship in 1859. Two years later, the area fell into Confederate hands, only to be recaptured in 1862 by Union forces. These military operations devastated much of the yard. Pensacola supported Union blockading forces later in the Civil War, but it was a small activity continually threatened by disestablishment throughout the rest of the nineteenth century.

The yard enjoyed a brief renaissance soon after the Spanish-American War that was halted in 1911 when a cost-minded secretary of the navy finally closed the yard. Two years later, however, Pensacola was designated the navy's central aeronautical station. Although the author does not pro-

vide extensive insight into the reasons behind the navy's decision to reopen Pensacola, he does demonstrate the major growth that ensued. By the end of World War I, the station was reputed to be the largest air training facility in the world. Pensacola continued to be a major installation throughout the 1920s due to the navy's need to train personnel for its growing air arm.

The author relates local naval events to the urban history of the Pensacola region and skillfully depicts the continual political efforts of civic and business groups to retain or expand the station. Some attention also is given to the significance of Pensacola within the context of overall naval history and the evolution of American diplomatic policy in the Caribbean.

The volume is based on wide use of official records, private papers, and published works. A surprising omission from the author's bibliography is Archibald D. Turnbull and Clifford L. Lord, *History of United States Naval Aviation*, which is a standard account of its subject.

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MAURINE WEINER GREENWALD. *Women, War, and Work: The Impact of World War I on Women Workers in the United States*. (Contributions in Women's Studies, number 12.) Westport, Conn.: Greenwood Press. 1980. Pp. xxvii, 309. \$27.50.

*Women, War, and Work* is one of the most valuable contributions to date to our understanding of women's role as workers in the American economy. Maurine Weiner Greenwald is concerned with how economic developments during World War I affected women's employment opportunities, their perceptions and expectations of work, and their relationships at their jobs. She examines the ways the expansion and rationalization of the corporate economy, the mechanization and subdivision of work processes, the development of scientific management and welfare capitalism, and the increasing economic role of government helped shape women's roles in the labor force.

Through case studies of women employees in railroad work, street railways, and the telephone industry, the author constructs a mosaic of women's wartime opportunities, day-to-day work experiences, and relationships with male co-workers and supervisors. She illuminates in rich detail the complex ways in which a woman's experiences were related to her race, skills, occupation, and the nature and extent of government involvement and unionization in her industry. She finds that women who took on nontraditional jobs, such as welders in railroad shops, fared very differently from their sisters in more conventional female employment, such as



secretarial work in railroad offices. Throughout, Greenwald resists the temptation to smooth diversity into facile generalizations or to pass judgment on the ambiguous roles that unions, government agencies, and feminist reformers played.

The collective portrait of working women that emerges in *Women, War, and Work* challenges the stereotype of female wage earners as docile and indifferent to their working conditions. During the war, women took advantage of new employment opportunities, astutely assessed their interests, and asserted themselves individually and collectively. Greenwald's argument that "women proved to be shrewd exploiters of the wartime economy" is fully convincing. I would have welcomed more discussion, however, on why women war workers did not fit stereotypic patterns.

The author also explores male-female work relationships—relationships sometimes characterized by mutual support but more often by antagonism. Her detailed account of how men feared female disruption of their work traditions helps explain the hostility with which unions frequently treated women.

Both white and black women benefited from wartime employment. Frequently, however, their gains were shortlived. The lines of sex and race segregation established before the war held fast. Women in nontraditional jobs endured harassment and, after the war, usually lost their jobs altogether. Why did occupational segregation patterns remain so entrenched? Greenwald does not offer a definitive answer. Significantly, however, her evidence suggests that such patterns were not inevitable. As her discussion of the wartime labor situation in Kansas City concludes, workers could sometimes surmount the barriers of gender rivalry. More often, however, the "iron-clad rule of custom" triumphed.

*Women, War, and Work* is an excellent contribution to American labor history. I sometimes wanted Greenwald to generalize more from her findings. As a successful attempt to document and explain the complex and varied connections between economic developments and women's experience, however, the book opens important new vistas for women's history and should serve as a model for future studies.

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RICHARD D. SCHEUERMAN and CLIFFORD E. TRAFZER. *The Volga Germans: Pioneers of the Northwest*. (Gem Books.) Moscow: University Press of Idaho. 1980. Pp. 245.

The Russian Germans can be numbered among those American ethnic groups about which a more mature scholarship has developed over the past two decades. Until the 1960s, information about this

oft-misunderstood element rested mostly on memoirs, chronicles, and a few not very accessible academic theses. A sign of new development was the founding in 1968 of the American Historical Society of Germans from Russia, which stimulated a growing monographic literature. Several books published during the 1970s represented steps toward a more comprehensive evaluation of the Russian Germans: the 1974 translation by LaVern J. Rippley and Armand Bauer of Richard Sallet's 1931 German-language work, titled in English *Russian-German Settlements in the United States*, Adam Giesinger's *From Catherine to Khrushchev* (1974), and especially the well-written volume by Fred C. Koch, *The Volga Germans in Russia and the Americas from 1763 to the Present* (1977).

Richard D. Scheuerman and Clifford E. Trafzer's treatment of the Volga Germans of eastern Washington may be added to this growing list of mature scholarship. Theirs is not a comprehensive history of the Russian Germans; the communities dealt with comprise perhaps one-tenth of them. But the work has considerable value as an attempt to account historically for the character of one ethnic group found in a specific geographical context. Scheuerman and Trafzer follow the antecedents of the eastern Washington Volga Germans from eighteenth-century western Germany to the Russia of Catherine the Great, through over a century of development along the Volga, and through their subsequent emigration under harsher Russian policies in the 1870s and 1880s to America—first to the Great Plains, then, under the inducement of railroad colonization agencies, to the Pacific Northwest.

This is a history of a people, not merely of its leaders and institutions. The authors are sensitive to the culture and folkways of the Volga Germans and to the interaction of their traditions with their successive environments over two centuries. Social scientists might wish for a more analytical approach to such matters as cultural change and the transfer of agricultural practices, but the authors do at least recognize the complicated variety of such possible influences as soil, climate, technological innovation, new economic opportunities, and the conditions of land tenure. The discussion of the character of the eastern Washington communities could have been enhanced, however, by some careful study of the manuscript census of 1900.

A final chapter dealing with religion among the Volga Germans seems somewhat out of harmony with the rest of the work. This is religious history in the more traditional, narrowly institutional sense, concerned with the founding of parishes and the memoirs of successive pastors; it does not penetrate as well as it might into the religious life of the community.

Scheuerman and Trafzer's work will be used by scholars of ethnicity generally, but it also is a valu-

able contribution to regional history, enabling residents of the Northwest to take a penetrating view at one of the more significant elements in their society.

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ANDREW ROLLE. *The Italian Americans: Troubled Roots*. New York: Free Press. 1980. Pp. xviii, 222. \$14.95.

For anyone familiar with *The Immigrant Upraised*, Andrew Rolle's uplifting early work on Italian-Americans, the present volume, as its subtitle *Troubled Roots* suggests, is depressing. I suppose this is inevitable because people bury their negative feelings, and it is these "buried impulses," "inchoate emotions," and "unconscious feelings" that Rolle searches out in this psychohistory (p. xiii). Rolle believes the psychohistorian's role is to analyze "the covert problems of persons in pain, individuals who are perplexed by their anxieties" (p. xiv). Though he claims his task is to understand complex behavior and not to overemphasize pathologies, it is difficult to get away from the illnesses. They are an integral part of his psychohistory. Similarly, Rolle's use of the language of psychohistory is disheartening and perhaps unavoidably leads him to paint an even bleaker picture of the immigrants' past than is justifiable.

Rolle's central theme is that some immigrants succeeded at the expense of uprooting and denial of the old ways. The upshot was "embittered" immigrants filled with "self-hatred" who masked their insecurities and their "history of shame" and nursed "irrational guilts" about abandoning their ties to their own pasts. They "buried" their "feelings of anger and fear" and put up "a rigid pattern of concealment" that "convincingly distorted" their history. In effect, one way that immigrants were able to survive their loss of identity was to deny "things psychological."

There is no doubting Rolle's academic credentials. Few have the erudition and command of the literature that he demonstrates. He has undertaken a pioneering effort that no serious scholar will want to ignore. His is an insightful understanding of Italian-Americans, in which old points are often seen in a new light. For example, on the legendary secrecy and humility of Italians, Rolle states that because peasants had to protect their valuables they came "to minimize one's status in life, to keep a low profile. America . . . called for a reversal of such attitudes, for Americans measured worth by the display of property and position" (p. 31). This demanded a role reversal that took its toll over the generations. The defensiveness and lack of self-pride the author alludes to rise more to the surface as scholars dig into life stories of individual immigrants. And what Italian-American family is without its woman accepting "a *mater dolorosa* role"? Indeed, the two

chapters on family are among the most interesting and most germane to the author's central theme.

Assuming Rolle's analysis is accurate, what does it all mean? How relevant is it for the history of millions of Italian-Americans? How many suffer "from an identity confusion based upon past unrealistic parental authority and the family's disrespect for learning" (p. 138)? Unfortunately, we cannot tell from Rolle's book. Though it has a sound bibliographical essay, its footnoting is sparse. Two chapters, for example, have one footnote each. In addition, the sources are mainly secondary and fictional. Conjecture is overabundant.

The author's writing is not always clear or well organized. His chapter on crime offers a case in point. While he seems at ease in dealing with Italian-American criminals and the Mafia, the reader is never sure of the direction of this chapter. The recent findings of scholars like Humbert Nelli, and especially Francis A. J. Ianni, are not well integrated in it.

There are other problems. The discussion of the *padroni* is inadequate. Rolle's reliance on traditional negative views causes him to see these ethnic leaders as "human leeches" (p. 61). All of them were not bloodsuckers. Some often provided material and spiritual nourishment that kept immigrants going and made it possible for them to prosper in America.

Furthermore, Rolle never fully explains why *il destino*, that fate beyond human control, prevented Italian-Americans from getting educations but was powerless to stop Italian-American laborers from embracing "hard-hearted" Yankee materialism when they so desired (pp. 30, 136).

As one may surmise, this is a difficult and troubling book. Its true value may not come to light for some time. Historians who will want to follow new paths charted by Rolle will have to figure out how to uncover "pervasive impulses carried along, unperceived, for generations" (p. 179). That, however, is easier said than done. The effort may be worth it. In its present state, this book is more psycho- than history.

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RICHARD M. BERNARD. *The Melting Pot and the Altar: Marital Assimilation in Early Twentieth-Century Wisconsin*. Minneapolis: University of Minnesota Press. 1980. Pp. xxviii, 162. \$17.50.

Arguing that recent scholarship on the structure and evolution of ethnic groups and communities leads to the tacit conclusion that the melting pot was indeed a myth, Richard M. Bernard demonstrates, in a careful study of intermarriage rates among Wisconsin's major ethnic groups, that the

melting pot was alive and bubbling in the pre-World War I era.

Unlike most studies of intermarriage, *The Melting Pot and the Altar* uses statewide sources rather than the records of a single city or county to examine its subject; thus, Bernard's findings offer a rural counterpoint to the urban-centered scholarship most commonly encountered. This focus proves valuable since, in fact, intermarriage was more common in Wisconsin's rural counties than in the cities. Such factors as the relative size, time of arrival, and occupational status of the members of the state's various ethnic groups are also examined with an eye toward their correlation with marriage trends. Here, Bernard's findings are interesting for their inconsistency; no single attribute, or cluster of attributes, accounted for the behavior of the members of Wisconsin's major groups. Always presented within the context of existing scholarship on its subject, consistently careful and modest in its claims, *The Melting Pot and the Altar* is a useful contribution to the literature of the American ethnic experience.

There is, however, a disappointment here. Bernard introduces his study by noting that most recent accounts focus closely on the viability of ethnic groups, communities, and institutions, not on intermarriage. Yet he fails to use his findings as a vehicle for reinterpreting our assumptions concerning the durability of ethnic separatism. In these pages, intermarriage seems simply to be an interesting but relatively inconsequential phenomenon. This unfortunate situation derives from Bernard's decision to stop his scrutiny of marriage patterns at the altar. Did intermarried couples separate themselves from their respective ethnic communities? Were they, in a sense, ethnic alumni, who had graduated from ethnicity? Or, perhaps, were they re-absorbed into one or another of the ethnic communities from which they had come? Such questions are amenable to examination; in a study of this type they ought to have been asked. With their omission, the impact and significance of intermarriage in a society premised on ethnicity cannot be assessed. While *The Melting Pot and the Altar* is a book that scholars concerned with the American ethnic experience will find useful, this reviewer would have liked to have seen such questions at least posed, even if definitive answers were nowhere to be found.

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DEBORAH DASH MOORE. *At Home in America: Second Generation New York Jews*. (Columbia History of Urban Life.) New York: Columbia University Press. 1981. Pp. xiii, 303. \$15.95.

Deborah Dash Moore has written the first volume in the "Columbia History of Urban Life" series. Completely conversant with the statistical and in-

terpretative methodology of the new history, she has provided us with an insight into the lives of immigrants' children. Unlike the plethora of volumes romanticizing the *World of Our Fathers* or the "gilded ghetto" of their grandchildren, *At Home in America* concentrates on the more prosaic pursuits of the second generation. Rather than finding a state of voluntary cultural amnesia among the subjects under investigation as compared to the return of the third generation to the ideals of their grandparents, Moore finds a continuity of values among the generations.

The thrust of the author is an examination of the institutions fashioned by second-generation New York Jews in the image of their new environment. Acculturation and social mobility were salient characteristics of that generation, which maintained a remarkable sense of ethnicity. Contributing to the assimilation of the children of ghetto parents was the public school. Unlike the contemporary experiment with the mechanism of foreign languages to buffer the transition from immigrant to American, public schools in the 1920s disapproved of the use of Yiddish, the major mode of expression of the East European Jew, and denigrated its cultural potential. While the former approach facilitated the entry of new Americans into society, it did create conflicts among the generations and fostered the neglect of traditional values necessary for the preservation of family, religion, and morality.

What fostered the reassertion of ethnicity among the second generation was the re-establishment, in new areas of New York City, of a residence pattern of voluntary segregation as had previously existed in the Lower East Side of Manhattan. The synagogue became Americanized as a function of middle-class needs with the goal of belonging supplanting the former manifestation of religious furor and nostalgia for the values of the old country. Political involvement of second generation New York Jews reflected urban needs and the desire for social reform rather than the doctrinaire politics of their immigrant parents.

Notwithstanding the impressive case that was made by the author for the contiguous neighborhood as a primary mold of ethnic values, she overlooks the factor of common identity or peoplehood as a creator of cultural values. Although she describes in great detail the influence of Mordecai Kaplan in the development of the synagogue-center, she could have profitably applied his definition of Judaism as a people-oriented religion to explain the genesis of ethnic values among second-generation Jews. That residential contiguity enhanced ethnicity is obvious but that proximity to fellow ethnics did not necessarily promote that degree of cohesion among other immigrant groups as it did among the Jews suggests that a new environment does not completely explain the reassertion of eth-

nicity among second-generation New York Jews. It would have been profitable for the author to compare the level of ethnicity of New York Jews living in such sparsely settled sections (in number of Jews) of the city, such as Queens, to Williamsburg. Another avenue of research is the family association histories now being collated by the Yivo Institute of Jewish Research. One weakness of the volume is the author's reliance on secondary sources for her conclusions. Certainly Rabbi Bernard Revel, then director of the Orthodox Yeshiva University, should not be cited as an authority on the curriculum of the Conservative Jewish Theological Seminary of America. I would also question Moore's explanation of the rationale manifested by the affluent German Jew in adopting philanthropy as his new religion. Did his desire for hegemony over the East European Jew emanate from the American concept of the stewardship of wealth or from the fear of the more assimilated classes that their immigrant coreligionists would constitute that degree of economic and social dependency that could generate a significant wave of antisemitism?

Nevertheless, the author is to be commended for her concentration on that area of research usually neglected by ethnic historians. We anticipate further contributions by the "Columbia History of Urban Life" series.

MYRON BERMAN  
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STANLEY LIEBERSON. *A Piece of the Pie: Blacks and White Immigrants since 1880*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xiii, 419. Cloth \$34.50, paper \$11.50.

Sociologist Stanley Lieberman attempts, in this provocative volume, to provide a more comprehensive answer than is usually given to the question: why did immigrants from central and southern Europe after 1880 fare so much better in the United States than blacks? Nagged by doubts regarding traditional answers emphasizing racism, skin color, legacy of slavery, and cultural deprivation, Lieberman undertakes an empirical analysis of various other factors largely unrelated to race that, he believes, operated to restrict opportunities for blacks and to widen the gap between them and white immigrants. Eschewing "the issue of inherent differences in intelligence between the races" as "a red herring" (p. 135), he devotes attention primarily to issues such as access to politics and education, demography, occupation, income, values, kinship structure, nuclear family stability, fertility, mortality rates, and residential segregation to determine whether they "are more than merely a part of the rhetoric of race relations."

Despite certain similarities between black and white immigrant experiences, the liabilities of the

latter were less substantial and disappeared sooner. Not only did immigrants have a "demographic headstart," they also possessed more access to the political process and confronted fewer barriers to labor union membership. Taken together these factors gave them highly significant advantages over blacks in economic and social areas. According to Lieberman, an important factor in the occupational and educational gap that developed between blacks and immigrants was the timing of the mass migration of blacks into the North. Arriving there in ever larger numbers during and after World War I just as the flow of European immigrants sharply declined, inexperienced blacks entered the labor market at a moment when the more experienced, upwardly mobile immigrants had pre-empted the skilled and semi-skilled jobs. The decline in black education and the increase in segregation, Lieberman argues, resulted not from a deterioration in the "quality" of black migrants but rather from "structural shifts in racial relations in the northern cities" (p. 219). Increasing black isolation was, in his view, the result of attempts by urban whites "to maintain the same degree of isolation from blacks that existed before the flows from the south started" (p. 291). That blacks were victims of more severe forms of discrimination than white immigrants, Lieberman concludes, was the product not so much of racial, as of economic, factors. At the heart of the matter was "the fear of blacks based on their threat as economic competitors" (p. 383).

The conclusions and implications of this volume are likely to prompt considerable debate. Specialists in United States history will be familiar with much of the ground that it covers, but not all will feel entirely comfortable with some of the author's arguments or with his so-called "elegant" methodological procedures. His references to "tautological demography," the queuing process, a "tautological approach to queuing," and other terms more often found in sociological literature will undoubtedly be alien to some. But because the volume "focuses largely on past events rather than current popular issues" (p. 15), anyone interested in the history of blacks, immigrants, race relations, and related topics will find Lieberman's approach enlightening and profitable.

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LORETTA J. WILLIAMS. *Black Freemasonry and Middle-Class Realities*. (University of Missouri Studies, number 49.) Columbia: University of Missouri Press. 1980. Pp. 165. \$15.00.

Few scholars of the Afro-American experience have given sustained attention to the role of fraternal or-



ganizations in black history. Charles H. Wesley, who wrote histories of the Improved, Benevolent, and Protective Order of Elks of the World, Sigma Pi Phi, Alpha Phi Alpha, and Ohio's Prince Hall Masons, was one of few professional historians to scrutinize these secret societies seriously. Although Wesley recognized the differences among each of these groups, he held that they all identified with the black masses and made major attempts to improve their social, economic, and political condition.

Recently, a few scholars have rediscovered black fraternal organizations as subjects for study. So far, these students of the Afro-American past have devoted most of their attention to Freemasonry among blacks. William A. Muraskin examined this phenomenon in *Middle Class Blacks in a White Society: Prince Hall Freemasonry in America* (1975). Shortly after the Muraskin book was published, Loretta J. Williams wrote *Black Freemasonry and Middle-Class Realities*.

Williams, a sociologist who teaches at the University of Missouri at Columbia, challenges Muraskin's claim that Prince Hall Masons maintained an exclusive bourgeois organization that remained aloof from lower-class blacks. She used the theory of pillarization, an analytic tool drawn from Dutch sociology, to contend that black Masons constituted a separate, parallel organization that shared the same Masonic heritage and ritual forms as their white counterparts. Williams observed, however, that significant differences existed between black and white Masonic groups. The development of Prince Hall Freemasonry paralleled the Afro-American experience. Black Masons were initially organized in 1787 by Prince Hall, a black Bostonian, because whites forbade blacks to enter their lodges. Black Masonic groups grew and flourished throughout the nineteenth and twentieth centuries because they served as a refuge for blacks whose social lives were restricted by racial segregation and discrimination. As a result of these realities, Prince Hall Masons identified with the struggles and aspirations of black people. They made no attempt to separate themselves from the fight for racial justice and equality. Although integration is now an achievable goal, Williams believes that the racial heritage of the Prince Hall Masons allows them to retain their appeal to blacks. They believe that they can be true to Masonic principles without amalgamating with their white counterparts. Although Masonic recognition is important to them, they do not seek Masonic integration. She concludes that blacks have preserved a separate, autonomous group without betraying the principles of Freemasonry.

The case that the author presents is convincing, but one flaw remains in her argument. She is unclear about her definition of the black middle class.

She discusses class divisions among free blacks during the antebellum period, but she does not identify the socioeconomic characteristics that Prince Hall Masons possessed. From 1865 to the present the author does not present any occupational or income criteria on which to determine what segment of the Afro-American population comprised the black middle class. The term is used indiscriminately throughout her study as though the composition of the black bourgeoisie remained constant through time. During the twentieth century Prince Hall Masons attracted black professionals and black laborers. Some worked as barbers, butlers, Pullman porters, and other high prestige occupations. Some were industrial laborers who found employment in coal mines, steel mills, and automobile factories. Does the author mean to imply that black physicians and black industrial laborers shared membership in the black middle class? These membership patterns suggest that the Prince Hall Masons and other black fraternal organizations cut across class lines to embrace black professionals and black workers in high prestige occupations as well as upwardly mobile black industrial laborers.

The Williams study is worth reading since it persuasively argues that black Masons identified with the struggles and aspirations of the black masses. It is not, however, the definitive work on Prince Hall Freemasonry and its role in Afro-American history.

DENNIS C. DICKERSON  
Williams College

SYLVIA M. JACOBS. *The African Nexus: Black American Perspectives on the European Partitioning of Africa, 1880-1920*. (Contributions in Afro-American and African Studies, number 55.) Westport, Conn.: Greenwood Press. 1981. Pp. xiv, 311. \$27.50.

This book makes a significant contribution to black history by focusing on Afro-American reactions to European imperialism. Nineteenth-century blacks were interested in their ancestral fatherland, but negative images absorbed from whites produced ambivalent reactions among educated blacks. Having imbibed Victorian notions of "civilization," these black journalists, clergy, educators, and other professionals initially supported European involvement in Africa. They were convinced that conversion to Christianity and Westernization was necessary before Africa could rise economically and politically. After the 1880s, however, several events led to a change of attitudes, so that by World War I most black spokespersons were anti-imperialist.

Sylvia M. Jacobs makes especially good use of black newspapers, church records, and diplomatic correspondence to prove her thesis. The poor organization of the book, however, obscures major points.

What is obviously missing is the hand of a good editor. Chapter 3 jumps in disarray from one person to another. Chapters 4 through 10 are awkwardly divided according to geography, with too much detail provided on European actions rather than on Afro-American attitudes. Chapters on East and North Africa are especially in need of condensing. This topic is admittedly difficult to organize, but since Jacobs suggests that black Americans only gradually turned away from imperialism, it is up to her to clarify the chronology of that change.

Reorganized into a chronological pattern, the growing Afro-American objection to European expansion becomes clear. First, in the 1880s and 1890s black United States diplomats began protesting British and French encroachments upon Liberia, which was an important symbol of black self-government. In 1896 Ethiopia, the other major independent black nation, was threatened by Italy. Afro-Americans exhibited pride that Ethiopia militarily repulsed the imperialists. Next, Jacobs brilliantly argues that black disillusionment with the American takeover in the Philippines marked the turning point in black attitudes toward imperialism. Black troops in the Philippines after 1898 observed that colonial control in practice led to racist exploitation rather than humanitarian uplift. Concurrently, the Anglo-Boer War publicized European racism, since British attempts to placate white settlers after 1902 led to increasing segregation and restrictions on Afro-American churches in South Africa.

It gradually dawned on black Americans that European control more likely held Africans down rather than benefited them. Nothing illustrated this more clearly than King Leopold's atrocities in the Congo. From 1904 to 1908, Afro-Americans exerted pressure on the United States to ask for reforms. That effort was followed by another successful black American initiative in 1911 to get the government to ensure Liberian independence.

Thus, by 1914 Afro-American comments on Africa were largely anticolonial, and blacks used World War I as an opportunity to attack German imperialism. Black journalists, educators, clergy, and the NAACP proposed an international commission to administer liberated German colonies while preparing them for self-government. More activists saw the status of black people in America and in Africa as inevitably tied together, as stronger bonds of pan-Africanism emerged.

This is a well-researched study, with only one notable factual error (on page 143, African Methodist Episcopal Bishop Henry Turner did not visit South Africa until 1898). Although disorganization detracts from the book's readability and Jacobs underestimates the role of Westernized Africans in influencing black Americans, her ideas and research

place her in the front rank of Pan-African historians.

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JAMES E. FICKLE. *The New South and the "New Competition": Trade Association Development in the Southern Pine Industry*. Urbana: University of Illinois Press, for the Forest History Society. 1980. Pp. xii, 435. \$17.50.

*The New South and the "New Competition"* is a history of the Southern Pine Association, 1915-54. The SPA was a successor of the Yellow Pine Manufacturers' Association, which was dissolved following the Missouri ouster suit in 1914.

To avoid possible prosecution under antitrust laws, the SPA was legally a private corporation. The association made no recommendations regarding prices or production to its subscribers who were manufacturers of lumber. Under the leadership of J. S. Rhodes and his successor, H. C. Berckes, lumbermen were kept up to date on all matters pertinent to the industry. Presumably, according to the views of Arthur Jerome Eddy, advocate of the "new competition," manufacturers with current information on products, distribution, and price could eliminate cutthroat competition. Southern pine manufacturers, however, because of a fear of antitrust prosecution and the existence of a large number of producers, were unable to establish uniform prices or to control output.

The SPA played a significant role in organizing its subscribers to support the country's efforts during World Wars I and II. In fact, the SPA came close to being an agency of the government. Despite controversies with Bernard Baruch of the War Industries Board, the SPA was commended during World War I for its contribution to the war effort. Because of shortages of labor and timber and confusing government regulations, lumber production declined during the Second World War.

The depression of the early thirties was nearly catastrophic for Southern lumbermen and the SPA. Consequently, the lumber code of the National Recovery Act established by lumbermen was welcomed. The SPA was designated as the agency responsible for administration of the lumber code. But supervision of twenty thousand mills, located in sixteen pine-producing states, proved impossible. Even before the NRA was held unconstitutional, the lumber code had been abandoned in the Southern pine industry. To Southern lumbermen the most objectionable aspects of the lumber code were maximum hour and minimum wage provisions and the unsatisfactory prices of lumber.

Lumbermen maintained almost a solid front in

opposition to formation of labor unions in the industry. Despite attempts of the AFL-CIO to unionize sawmill workers, only slight success was achieved. Moreover, wage and hour provisions of the Fair Labor Standards Act met universal disapproval from industry owners.

In 1954, the able H. C. Berckes, because of changing conditions in the industry, resigned as secretary-manager. Over the years a new breed of leader, the managerial type representing a growing diversity of the industry, gradually took control. Ultimately the Southern Forest Products Association superseded the SPA.

The SPA provided a classic example of the New South ownership patterns and attitudes toward government, labor, and society. Moreover, the association demonstrated a remarkable struggle between unfettered competition and the "new competition" pioneered by Eddy.

James E. Fickle is to be commended for his use of primary and secondary source materials. He has written a highly readable book that should be read by those with an interest in trade associations, the lumber industry, and business history. If the book has any major defect, it is in the organization of the subject material.

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Northeast Louisiana University

PHILIP T. ROSEN. *The Modern Stentors: Radio Broadcasters and the Federal Government, 1920-1934*. (Contributions in Economics and Economic History, number 31.) Westport, Conn.: Greenwood Press. 1980. Pp. 267. \$25.00.

Philip T. Rosen has a story to tell. It is a "search for order" story about the development of the American system of broadcasting in the formative decade of the 1920s. The story begins with chaos in the radio world: competing local stations, confusion over radio's proper purpose, government rivalry between the navy and post office and commerce department for the control of radio. To end this chaos the story's hero emerges in the figure of Herbert Hoover. As secretary of commerce, Hoover was "decisive" and "pragmatic" in shaping policy. Like a frontier hero, he went beyond legal restraints to solve the problem. He usurped the 1912 Radio Act and allocated channel frequencies to eliminate weak stations. He opened up the government wavelength reservation to private enterprise. He organized national radio conferences to formulate a policy of government regulation that became the Radio Act of 1927.

Although Hoover is the central character and the creation of the 1927 act the main plot, other characters and subplots contribute to the story's develop-

ment. The radio industry developed its own solutions to the problem of chaos. AT&T and Sarnoff's RCA began sponsored entertainment, continuous operation, and national network service. NBC and CBS developed techniques to dominate the medium after 1927. The influence of national advertisers such as the American Tobacco Company and Pepsodent grew enormously and determined the content of programs. The subplot of network and sponsor influence is tantalizing, but only a subplot. The reader yearns to learn more about the "advertising oligarchy which in effect allowed businessmen to determine entertainment programs and policies for government overseers." Instead, the main plot focuses on the positive role of government, especially the success of the Federal Radio Commission in implementing the 1927 Radio Act. The story has a dramatic climax with the "abortive revolt" of the small noncommercial stations. Hero Hoover (now president) stepped in to crush the revolt and pushed through legislation that eventually created the Federal Communications Commission. The free enterprise system was saved once more by a benevolent government.

Rosen writes his story well, supporting it with extensive archival research. His emphasis on the positive role of government improves upon the traditional story of radio developing in spite of government. What weakens the story is the "search for order" approach. The rich variety of human motivation and historical causation is never allowed to make itself known. Hoover has no ideology, only a lust for power. Paley and Sarnoff are interchangeable as network oligarchs. The desire for order and stability is the prime motive shared by all. Perhaps historians need to learn from the novelist that, in the best stories, the plot thickens rather than thins.

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DONALD R. RAICHLE. *From a Normal Beginning: The Origins of Kean College of New Jersey*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1980. Pp. 432. \$20.00.

In recent years some graduate schools in history have shown more interest in classroom instruction, but traditionally our craft has stressed substance over method at the college and university level; we have usually looked down upon pedagogy. And revisionist historians in their turn have attacked the American public school system itself. So we must welcome the informed views of a professional historian on the faculty of a New Jersey institution that has evolved from a city normal school to state

teachers college to multipurpose liberal arts college over the last 125 years.

Donald R. Raichle is far from an apologist for the ways in which Newark and New Jersey trained public school teachers, yet he does take exception to some revisionist contentions. He praises those progressive "normalists" who advocated more and better instruction in the liberal arts and sciences, yet he is sensitive also to the merits of a principal of the 1900-30 period who practically ignored John Dewey and fostered a "Sunshine School" atmosphere. He directs his sharpest criticism at an authoritarian state commissioner of education who resisted thoroughgoing reform in the 1950s and 1960s, but he recognizes the personal losses in faculty relationships that accompanied expansion and specialization under New Jersey's Higher Education Act. Though he strongly sides with the "academics" against the "educationists," in short, Raichle can still appreciate good things about the old order.

Research for this massive volume included a number of important interviews during the 1970s with administrators, faculty, and graduates, as well as a meticulous investigation of unpublished and published sources. And though the resulting study is primarily an administrative history that highlights the deeds and personalities of chief officials, local and state, it does touch upon teachers at points and more especially upon students and extracurricular affairs during and after the 1920s. Among similar institutions in New Jersey only Glassboro State College has published a comparable account, by Robert D. Bole, brought out for its semicentennial in 1973.

Raichle cuts off his story at 1969, when the much renamed New Jersey State Teachers College at Union became Kean College of New Jersey and Nathan Weiss became the first academic ever to head the institution. Eventful years were immediately ahead, yet for historical perspective alone it was no doubt wise to stop there. What is more, prospective readers may well be put off by the sheer mass of detail. A more critical editor could have reduced the bulk and sharpened the analysis. And surely a 16-page signature for "A Pictorial Chronology" seems a niggardly illustration for such a weighty text.

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ROBERT B. HIGHSAW. *Edward Douglass White: Defender of the Conservative Faith*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1981. Pp. xiv, 212. \$20.00.

Edward Douglass White (1845-1921) was the first associate justice of the United States Supreme

Court to be elevated to the center chair. He was eminently more successful as an administrator than the only other associate to become chief justice, Harlan Fiske Stone.

Robert B. Highsaw traces White's early years in Louisiana and his Catholic education, which culminated in legal studies at Georgetown University. After Civil War military service intervened to prevent young White from completing his degree, he read law and was admitted to the bar. He became involved in Reconstruction politics and was appointed to the Louisiana Supreme Court. He returned to Washington in 1891 first as senator and then as a member of the United States Supreme Court. His nomination in 1894 to the high court was as much a surprise to him as to his fellow Washingtonians. His appointment was a compromise to end a patronage feud during which Senator David Hill of New York had invoked senatorial courtesy, usually limited to federal appointments within a senator's own state, to block two of President Grover Cleveland's Supreme Court nominations.

Highsaw's subtitle, *Defender of the Conservative Faith*, prepares the reader for the thrust of the main chapters. White's views on states' rights and the expansion of federal power reflect his nineteenth-century Southern Democratic background, although the author is careful to point out the justice's inconsistencies in these areas. White's reluctance to use the Tenth Amendment as a fence against the expansion of the federal taxing power presaged the Hughes court's similar refusal to limit the interstate commerce power (*United States v. Darby* [1941]).

During the discussion of White's position on civil rights and liberties, Highsaw's analysis raises the dilemma that the true conservative faces in attempting to balance governmental interference and constitutionally protected civil liberties. A case in point is the chief justice's opinion in a contempt of court versus freedom of the press case, *Toledo Newspaper Co. v. United States* (1918). White wrote, "However complete is the right of the press to state public things and discuss them, that right as every other right enjoyed in human society, is subject to the restraints which separate right from wrong doing" (p. 154). Incidentally, the ruling was overturned by a liberal Democratic court in *Nye v. United States* (1941).

White's tenure on the court came to an end with his death in 1921 following surgery that he had postponed under the pretext that he was needed on the court. As usual, death proves that people are not as indispensable as they believe.

This reviewer found the chapters given to the Louisiana lottery issue and to the *Insular* cases, despite the author's attempt to justify this latter discussion, burdensome. In addition, there are two



points on which issue can be raised. The author seems willing to accept the unsupported statement of Thomas Dixon as the sole basis for the claim that White had been a member of the Ku Klux Klan. Dixon should be considered biased since he was the distributor of the film *Birth of a Nation* and wished to use the favorable comments of the chief justice to promote his movie. Given the Klan's anti-Catholic activities, the accusation seems difficult to reconcile with White's religious affiliation. Secondly, and more importantly, the judgment that the chief justice's defense of the conservative faith had greater effectiveness than that of the "Four Horsemen" of the 1921-36 court is subject to challenge.

Since this is the first biography of Edward Douglass White, it can only be judged against the background of judicial biography generally, rather than other works on the subject. Except for some articles, two unpublished doctoral dissertations represent the only other studies of the chief justice. Highsaw's volume merits a reading by the students of the Supreme Court, especially those whose interest lies in the period when *laissez faire* served as the national judicial philosophy.

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H. N. HIRSCH. *The Enigma of Felix Frankfurter*. New York: Basic Books. 1981. Pp. x, 253. \$14.95.

History has turned in the last quarter-century to the compilation of psychobiographies. H. N. Hirsch's study of Felix Frankfurter is a solid contribution to this developing genre. Calling Frankfurter an enigma expresses immediately what Hirsch acknowledges in his preface. His valuable work interprets but does not pretend to exhaust the multi-talented personality of the immigrant Austrian Jew, the brilliant legal stylist who could not speak English until he was twelve years old.

The periods of Frankfurter's life, appropriate chapter headings, place him in his time: his cherished professional preparation at Harvard Law School; his early political involvement with Henry L. Stimson and New York politicians; his choice of law practice "without clients" in the United States Attorney's office in New York; his involvement with Zionism just after World War I; his professorship at Harvard Law School; his friendship with and influence upon Franklin Delano Roosevelt; and his eventual appointment by that president to the Supreme Court. At the time of that appointment in 1939 Frankfurter was fifty-seven; he hesitated for some time about his acceptance.

That hesitation was one of the marks of his style—Hirsch uses the term to sum up Frankfurter's complex relationship with words, work, and people.

His life stretched between his own (often unrealistic) assessment of his abilities and his need to praise those who could benefit him. Adulation, cajolery, sycophancy, servility, flattery, and actual praise are all exhibited.

Frankfurter's relationship with Oliver Wendell Holmes, Jr., is here enlightening. Holmes relished flattery, from Harold Laski as well as from Frankfurter, never indicating that he recognized it as fulsome. He practiced it himself, as his lengthy correspondence with Sir Frederick Pollock shows. But what Frankfurter—and Laski as well—never perceived was that Holmes would not admit them to his true inner circle. They were not eligible, for what Frankfurter sometimes called "racial" reasons.

Did Frankfurter's calling himself "the kid" at thirty indicate a repressed awareness of his small stature or of his non-WASP background? Aside from his admittedly erratic spelling, why did he misspell Henry Morgenthau's family name? Are these clues to Frankfurter's narcissistic neurosis or to some other psychological quirk?

Similarly, Frankfurter's efforts to subdue, dominate, and repress his gentle wife merit reflection: were they an unacknowledged effort to prevent the recurrence of a strong feminine presence in his life, as his mother's had been? Or an unwillingness to allow Marion Denman to forget what a privilege she had received in becoming Marion Frankfurter?

Such considerations play a considerable part in Hirsch's portrayal of Frankfurter as associate justice, although the ones just mentioned are not Hirsch's explications. Frankfurter, self-styled intimate of Louis D. Brandeis and Holmes and an informed expert on the court due to his network of former students serving as clerks to many justices, met the "new boy—old boy" reaction. Frankfurter believed he knew the court as no other could; he resented having to undergo a probationary period until his colleagues would accept him as one of their brethren. He chafed under their reserved inspection and took his revenge in condescending instructorship. It failed, as did his intransigent commitment to the doctrine of judicial self-restraint. Admirable though the doctrine might have been, Frankfurter could not sustain it against fresher, often more controversial ideas.

Hirsch's occasionally exiguous vocabulary and roughness of style are minor matters. His diligent, well-informed research into Frankfurter's words, his penetrating review of Frankfurter's works, and his illumination of Frankfurter's dealings with people are meritorious indeed. He does service to the evaluation of Holmes. He has been faithful to his sources and to psychologists such as Karen Horney. Hirsch suggests interpretations that should be pursued profitably for other justices.

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NELSON LLOYD DAWSON. *Louis D. Brandeis, Felix Frankfurter, and the New Deal*. Hamden, Conn.: Archon. 1980. Pp. viii, 272. \$19.50.

This reviewer recalls well the mad scramble as researchers very nearly plundered the newly opened papers of Felix Frankfurter at the Library of Congress Manuscripts Division a decade or more ago. There were doubts concerning the reigning "two New Deals" hypothesis fashioned, I believe, by Raymond Moley in order to explain his break with Franklin D. Roosevelt. Too, Moley's papers seemed to suggest that Frankfurter was of minimal significance in the 1930s except as placement officer for graduates of the Harvard Law School. As was true of other aspiring New Deal specialists, I had already labored in the Brandeis vineyard at the Louisville Law School only to conclude that his handwriting derived from Sanskrit and once deciphered yielded little of substance for an understanding of the 1930s. These are the issues treated in the monograph under review.

Nelson Lloyd Dawson contends that had Franklin D. Roosevelt given greater play in the depression years to Louis D. Brandeis's philosophy, the economic crisis of the 1930s might well have abated. Brandeis is represented as a pre-Keynesian thinker, lacking in a social philosophy designed to cope with the depression but possessed of "a specific response to the economic crisis," one that was bold and innovative (pp. 28-29). I confess failure to grasp this argument, one that characterizes this awkwardly written and intellectually disappointing work. Lacking an adequate research base for analysis of depression causation, the author, like Brandeis, is compelled to substitute insistent repetition of proposals that, contrary to his insistence, are well known to a generation of historians: federal funding of a massive public works program (in turn financed by heavy taxation of large fortunes, incomes, and corporate aggregates); an overall attack on bigness, especially in the form of the holding company; and, in the manner of Ogden L. Mills and Herbert Hoover, reliance on the states for provision of the social minima (an unlikely proposition). This is represented five decades later as a new interpretation of the era. The New Deal in fact subsumed the Brandeis proposals and transcended them in its attack on the economic dislocations that had emerged in the previous decade. Brandeis, indeed, was principally concerned, as the author demonstrates, with scale, whether manifested in the private or public sectors, and as time passed became disenchanted with programs that moved the nation toward federal overhead management.

Dawson is at his best in his elaboration of Felix Frankfurter's conversion from his early Hamiltonian, anti-Wilsonian views to atomism as his friendship with Brandeis deepened. The author rejects

Frankfurter's reminiscent denial, expressed to Arthur Schlesinger in 1963, that he saw "completely eye to eye with Brandeis on socio-economic matters" and that he clashed with proposals of Moley and Tugwell as the New Deal took shape. Frankfurter is represented as an opponent of the economic concentration described approvingly by Adolf A. Berle as a permanent feature of our capitalistic development. Yet in the process we are compelled to ignore Frankfurter's letter to Geoffrey Parsons of the New York *Herald Tribune* (April 13, 1933): "I am not for a planned society *en gros*. I am prepared to get there by *ad hoc* treatment of specific problems" (in reference to TVA). Of the Roosevelt entourage, only Rexford Tugwell ventured, or at least aspired, beyond this position. Frankfurter comes across in the end as an intellectual trimmer, at times a conveyer of ideas who was basically willing to compromise views and was poorly informed on many fundamental economic problems, especially agriculture.

This brief work concludes by taking to task a generation of New Deal historians, the reviewer included, for casual dismissal of Brandeisianism as antediluvian in its approach toward complicated realities, those forces that constituted the fundamentals of depression causation. I remain unreconstructed and unconvinced. "Isaiah," as FDR was wont to describe the aged Jewish Brahmin, proved privately and in several critical Supreme Court decisions an old-fashioned Progressive and in the last analysis an opponent of much of the New Deal program. This is really the unintended message of Dawson's work. Wedded to outmoded panaceas and antistatist values, Brandeis preferred nineteenth-century verities to the appearance of modern corporate and financial structures and the omniscient state. To argue that Brandeis embodied pre-Keynesian modernity is a contradiction in terms.

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HUGH DE SANTIS. *The Diplomacy of Silence: The American Foreign Service, the Soviet Union, and the Cold War, 1933-1947*. Chicago: University of Chicago Press. 1980. Pp. x, 270. \$23.00.

*The Diplomacy of Silence* is first-rate diplomatic history. The Society of Historians of American Foreign Relations presented Hugh De Santis with the 1981 Stuart Bernath Prize for this book, and they knew what they were doing. De Santis has tackled an important and devilishly difficult problem, namely, what effect did the perspectives of the Soviet experts in the Department of State have on the origins of the Cold War? Other researchers, including me,

have tried to explain the importance of the foreign service to the conduct of American foreign policy, but no one has told the story as well as De Santis.

He begins by challenging the interpretations of the foreign service officers' outlook on Russia provided by Lynn Etheridge Davis in *The Cold War Begins* (1974) and Daniel Yergin in *Shattered Peace* (1977). Davis claimed that the professionals advocated a policy of cooperation with the Kremlin, while Yergin asserted that the diplomats consistently advocated a tougher line. De Santis demonstrates that both interpretations miss the subtleties of the diplomats' attitudes. He isolates at least four different diplomatic images of the Soviet Union. (1) Some believed in "ideological cooperation" with Moscow. (2) Others held to a pattern of "ideological confrontation." (3) Then there were "realists" who thought "realistic cooperation" was the best course. (4) Finally, there was a group of other "realists" who predicted a period of "realistic confrontation" with the Soviet Union.

The bulk of *The Diplomacy of Silence* consists of De Santis's explanation of how the professional diplomats developed these notions of the future of American-Soviet relations. He describes the careers of thirty professional foreign service officers before they served in Eastern Europe and the Soviet Union and while they were stationed there. He pays particular attention to the cultural environment of the foreign service—the diplomats' sense of being an enlightened group of experts who could put into practice the grand internationalist schemes of Woodrow Wilson. De Santis also points out the personal and psychological needs of the professionals. Their morale hit bottom during the Second World War when President Roosevelt made fun of them when he did not ignore them. Their sense of self-esteem shot up when Harry Truman took over. Insecure in his own grasp of foreign affairs, Truman relied on the advice of the experts, made them think they mattered, and greatly expanded the importance of the relationship with the Soviet Union.

Still, De Santis points out that it took at least a year after Truman became president before all of the foreign service experts on the Soviet Union could agree on the need for "realistic confrontation" with the Russians. In the process they had to shed many of their Wilsonian assumptions about cooperation and collective security. Since the professionals changed their minds as time went on, De Santis has noticed that they were as much followers of events as makers of foreign policy. Hence his title, which highlights their "silence." In fact, De Santis means that the diplomats were more quarrelsome than quiet. The effect, however, was the same. No one could detect a clear message from what the diplomats had to say.

De Santis has read all of the relevant published

and unpublished sources, and he was able to interview seventeen of the diplomats who are the book's subject. Furthermore, the author is well versed in the literature of existential psychology, a discipline he usefully applies to the diplomats' image of reality. Best of all, De Santis writes clearly, modestly, and gracefully. His book deserves to be read by everyone interested in diplomacy and the Cold War.

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ROBERT A. DIVINE. *Eisenhower and the Cold War*. New York: Oxford University Press. 1981. Pp. ix, 181. Cloth \$14.95, paper \$3.95.

The four essays that make up this volume represent an excellent introduction to recent interpretations of Eisenhower's foreign policies. The first examines Eisenhower's style of leadership, his relationship with John Foster Dulles, and his actions in bringing the Korean War to a close. The second traces Eisenhower's embrace of massive retaliation, his use of nuclear brinkmanship during the crises over the Chinese offshore islands, and his decision against military intervention in Indochina in 1954. The third essay focuses on the Middle East—the 1953 overthrow of the government of Iran, the Suez crisis of 1956, and the 1958 intervention in Lebanon. The final essay explores Eisenhower's search for peaceful coexistence with the Russians and his attempts to negotiate a ban on the testing of atomic weapons.

Like other recent historians of the Eisenhower presidency, Robert A. Divine argues that Eisenhower was a far more dominant and purposeful figure than most contemporaries believed and that it was Eisenhower, not Dulles or other advisers, who shaped American foreign and military policies during the 1950s. It is also clear from these essays that Eisenhower shared the same Cold War premises that animated the administration that preceded his, as well as those that followed. What made him different was his cautious and pragmatic cast of mind, his personal self-confidence, and the care with which he applied the means and ends test to his actions as president. He could, when risks seemed limited, act with ruthless efficiency in pursuit of what he believed to be national interests, as in Iran, Guatemala, and Lebanon. He could also act with considerable restraint when the risks seemed higher and the dangers of miscalculation greater, as in the Suez crisis and the confrontation over Berlin. "Nearly all of Eisenhower's foreign policy achievements were negative in nature," Divine concludes. "He ended the Korean War, he refused to intervene militarily

in Indochina, he refrained from involving the United States in the Suez crisis, he avoided war with China over Quemoy and Matsu, he resisted the temptation to force a showdown over Berlin, he stopped exploding nuclear weapons in the atmosphere" (p. 154).

Not all of his efforts were, of course, successful; as Divine concludes, he left behind a "mixed legacy." Perhaps more importantly, not even his "successes" will always bear close scrutiny. The intervention in Iran, for example, turned out to have disastrous long-run consequences for American interests in the Middle East. So, it may well be, did his summary actions in Latin America. Divine praises Eisenhower's use of brinkmanship and the "deliberate ambiguity" with which the president veiled his threats to wage nuclear war over Korea, the Chinese off-shore islands, and Berlin. "The beauty of Eisenhower's policy is that to this day no one can be sure whether or not he would have responded militarily to an invasion of the offshore islands, and whether he would have used nuclear weapons" (pp. 65-66). The obvious danger of such a tactic, of course, is that it also increases the chances that one's opponent may miscalculate and thus also increases the chances of atomic war.

If, finally, Eisenhower displayed restraint by refusing to intervene militarily in Indochina, it was a restraint produced more by France's refusal to grant its colonies full independence and allow the U.S. a decisive role in the military conduct of the war than by any particular reluctance on Eisenhower's part to employ force in international affairs. As the president wrote a close friend in October 1954, he had been unable to obtain "the conditions under which I felt the United States could properly intervene to protect its own interests" (Eisenhower to E. E. Hazlett, October 23, 1954, box 18, Ann Whitman File, Dwight D. Eisenhower Papers, Dwight D. Eisenhower Library). He was nevertheless committed to drawing the line in Southeast Asia. "We have got to keep the Pacific an American lake," he told his advisors (Conference in the president's office, June 2, 1954, box 11, Administration series, Ann Whitman File, Eisenhower Papers). Eisenhower committed the U.S. to the support of the Diem regime in South Vietnam and effectively undermined the international agreements reached at Geneva in 1954. These actions, as much as any, opened the door to expanded American involvement in the decade that was to come.

If Eisenhower now seems so good, perhaps this is largely a measure of how badly our leaders have performed since 1960.

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ROBERT A. PASTOR. *Congress and the Politics of U.S. Foreign Economic Policy, 1929-1976*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xiii, 366. \$24.50.

Robert A. Pastor has written a book that attacks the received model guiding most of the previous research depicting the making of foreign economic policy. The accepted assumptions dominating research since E. E. Schattschneider's study of the Smoot-Hawley Tariff of 1930 assert that special interests on behalf of discrete constituencies motivate the majority in Congress and that this political reality prevents the enactment of a rational foreign economic policy (p. 70). Pastor also successfully attacks the view that bureaucratic power struggles over "turf" have stood in the way of developing a rational policy. Yet Pastor's work does tend to sustain interbranch (executive-congressional) power struggles as establishing the basic framework within which such policies are made. But he rejects the traditional view that the struggle is between a protectionist Congress and a free trading executive. He sees 1930 as the last time during which Congress was essentially motivated by protectionism in making trade policy. Subsequent to 1934, both the executive and the Congress were motivated by an effort to obtain "fair trade" treatment for the United States by other nations. Congress and the executive were not struggling over free trade versus protection but over executive efforts to ignore ("failing to consult," and hence acting unilaterally without) congressional input in making foreign trade policy (pp. 198-99).

In terms of methodology Pastor has selected certain landmarks to examine. These were trade policy (from 1929 forward), foreign aid policy (from 1945 into the 1970s), and foreign investment policy (from 1961 into the 1970s). He asks of a series of cases, derived from the secondary literature, whether or not a case in any one of the three categories exemplified a liberal or illiberal approach to trade and investment? Aside from his legislative case material, his basic sources were interviews with some "two hundred governmental officials from Congress and the executive branch . . . and . . . from non-governmental organizations," who, unfortunately, "requested confidentiality" (p. 355).

I would agree that he has corrected the Schattschneider model. This is the book's greatest strength. But it has serious weaknesses. In part those weaknesses inhere in the case study method, which justifies failure to use manuscripts. It may well be that he has read the published sources "correctly," and that he has not been "tricked" by his "interviewees," but it would have been somewhat more convincing had he looked into the manuscript sources open to him, such as the Dulles, Roosevelt,



and Truman papers, thus checking published against unpublished sources for consistency by such sampling.

Perhaps a more important weakness is the fact that Pastor has excluded the areas of foreign trade policy that tend to show the Roosevelt administration as "illiberal." The Roosevelt administration refused to attend the London Economic Conference of 1933, where an international currency stabilization program among the industrial nations was to be discussed in order to avoid competitive currency depreciations designed to steal one another's markets. Indeed, the New Dealers went ahead and both tried to restrict European exports to the United States and to steal markets from the other industrial nations by devaluing (depreciating) the dollar by more than 40 percent. In addition, Pastor fails to mention the fact that the Roosevelt administration inaugurated the policy of U.S. government subsidies for exports when it had Congress create the Export-Import Bank of Washington in 1934. Thus reciprocal trade agreements cannot be interpreted unambiguously as a commitment to free trade when export subsidies and currency depreciation are being used at the same time to restrict the trade of other nations. Ignoring these paradoxes does not resolve them. This is a good book, which we should all read, but it is not *the* book of Congress and United States foreign economic policy, 1929 to 1976.

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JUAN RAMON GARCÍA. *Operation Wetback: The Mass Deportation of Mexican Undocumented Workers in 1954*. (Contributions in Ethnic Studies, number 2.) Westport, Conn.: Greenwood Press. 1980. Pp. xviii, 268. \$25.00.

This book delivers much more than is promised by the narrow focus of its title. Juan Ramon García reaches back to the origins of the bracero program to survey the course of American immigration policy toward Mexico, its inconsistencies and ineffectiveness, and the remedy that cured illegal immigration in the public mind while achieving dubious tangible results.

From 1942 to 1964, United States immigration policy toward Mexico was molded by the economic and political concerns of Southwestern growers. Determined to keep labor costs down, these growers insisted that only Mexican farm labor could provide the necessary number of workers at a wage they could afford to pay. The Mexican government attempted to secure guarantees of equitable treatment for its braceros but was seldom successful in doing so. A noticeable gap always existed between

pledges on paper and conditions of reality. Many Mexicans desiring to escape from poverty bypassed the bracero program. Without much difficulty, they moved across the border as undocumented workers, finding employment but also exploitation and disillusionment.

In view of current arguments concerning the entry of undocumented workers into the United States, this book demonstrates clearly how the border functioned more as a sieve than an obstacle. The border patrol was viewed with contempt not only by those who entered illegally but also by Southwestern growers who considered the patrol a laughable irritation. Its budget periodically cut, the patrol found itself undermined and underpaid. As often as not, it was used to transport undocumented workers to the border to "legalize" them into braceros so that harvesting could be completed on schedule.

In 1954, while Congress stalled on punitive legislation for employers of undocumented workers, Attorney General Herbert Brownell approved a highly publicized deportation campaign to remove Mexicans illegally in the United States. This campaign, known as "Operation Wetback," supposedly deported over a million Mexican aliens from the country. García argues convincingly that the campaign did not accomplish what it claimed—the numbers were greatly overinflated, penalty legislation died in committee, and the basic reasons for emigration from Mexico were not addressed.

Historians will find this book more useful than Richard Craig's *The Bracero Program*, which approached the topic from a political science viewpoint. Although García's sympathies lie clearly with the undocumented worker, his dispassionate presentation allows the facts to speak for themselves. He finds the terms "wetback," "illegals," and "illegal alien" personally distasteful, but he recognizes the necessity of using these terms, since individuals and groups have used them in the past. Throughout the book he places them in quotation marks, an interesting way of expressing his feelings on a sensitive issue.

In matters of format, the author has not been especially well served by his publisher. Unjustified right-hand margins, a minuscule typeface, and numerous typographical errors in text and notes test the reader's concentration. The rather unattractive packaging should not, however, deter scholars from making use of the valuable information this book provides.

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HONORÉ M. CATUDAL. *Kennedy and the Berlin Wall Crisis: A Case Study in U.S. Decision Making*. (Political Stud-

ies, number 17.) Berlin: Berlin Verlag. 1980. Pp. 358.

This book, the first in a multivolume series, traces the development of the Kennedy administration's Berlin policy from January 1961 until the closing of the sector boundary in mid-August 1961. The book is motivated by two principal concerns: first, to provide a detailed account of the events of the crisis as an aid to future historical and analytical accounts; and second, to illustrate the usefulness of the "rational actor" model of decision-making analysis.

Toward these ends, Honoré M. Catudal has conducted a thorough search of archival and primary source materials in the U.S. and West Germany. He interviewed and corresponded with many of the key participants. He has also mastered the secondary literature on the crisis. His efforts in this regard allow him to present important new information on the formulation of U.S. policy during the crisis.

Catudal's substantive focus is on John Kennedy's perceptions and choices. "Contrary to what many researchers in the field of decision-making often assume," he argues, "this case study suggests that rationality (however construed) may indeed play a crucial role in how decisions are reached" (p. 11).

Despite the wealth of new material available to him, Catudal's book is nonetheless disappointing. The prose is often awkward and contrived, and the narrative is cluttered with trivial and irrelevant material. The tone is at times condescending: John Kennedy is repeatedly referred to as "the young man" and Robert Kennedy is dismissed as an "eager beaver" (p. 73).

More disturbing is the way Catudal handles his evidence. He often describes the thoughts and feelings of John Kennedy and other participants in the crisis. These descriptions are generally accompanied by footnotes that describe the sources that allowed Catudal to peer into the minds of others, but sometimes they are not. Key points in the argument are attributed to an "informed Soviet source" (p. 66) and to information gathered by "Western intelligence services" (p. 209) without the slightest hint as to how Catudal gained access to this material. The account of Soviet and East German decision making relies heavily on testimony from a Czech defector, Jan Sejna, even though Catudal quotes an anonymous "intelligence analyst" as dismissing Sejna as a "crass opportunist" who "peddled a lot of nonsense" (p. 50).

Finally, Catudal's evidence does not always support the interpretation he attempts to impose on it. He describes Kennedy's major concern as being to "prod the bureaucracy into action" (p. 139). In so doing, he raises questions about his understanding of how the government operates. The bureaucracy is always "in action," drafting papers, clearing ca-

bles, updating contingency plans, and drawing up recommendations and proposals for senior officials to consider. From the president's perspective, the problem is not to prod his subordinates to do things, but to do the *right* things while refraining from doing the wrong things. In the Berlin crisis, Catudal's evidence portrays a president concerned not with energizing a sluggish bureaucracy but rather with channeling those activities already under way in directions that would demonstrate resolve to defend vital Western interests in West Berlin while avoiding provocative steps that would challenge important Soviet interests in East Berlin.

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CLAYBORNE CARSON. *In Struggle: SNCC and the Black Awakening of the 1960s*. Cambridge: Harvard University Press. 1981. Pp. viii, 359. \$22.00.

In Clayborne Carson the Student Nonviolent Coordinating Committee (SNCC) has at last found a scholar capable of probing its radical and fractious nature in a manner both sympathetic and prudently critical. The work is tightly structured, extensively researched, and accomplishes what the author set out to do: to study "the ideas that came to life inside SNCC" and to trace its development "through three stages" (p. 2). Since Carson did not propose to produce a social science analysis of SNCC, he was not constrained by an elaborate conceptual model of analysis. The descriptive mode appears to be more congenial to the author than the analytical, yet he does not fail to make value judgments when they are called for, without, however, forcing his interpretations upon the objective data.

Although F. W. Maitland spoke of history as a "seamless web," Carson depicts the history of SNCC as more convoluted than weblike, and divided both ideologically and tactically. Like those historians who chop their subject into three chunks—ancient, medieval, and modern—Carson divides SNCC's history into "Coming Together" (part 1), "Looking Inward" (part 2), and "Falling Apart" (part 3). At the end there is a two-page epilogue that brings the colorful careers of SNCC's leaders up to date. In a brief introduction and eighteen chapters, Carson deals evenhandedly with a vast array of controversial issues and rarely lets his sympathy for SNCC vitiate his balanced approach. To a significant degree the book is a collection of sharp miniatures of men and events, interspersed with a number of keen insights into the nature of social change and racial violence.

Despite the fact that many of Carson's findings merely confirm generalizations from earlier studies,

his book meets the critical need for a comprehensive, well-organized, and impartial analysis of the part played by SNCC in the black awakening of the 1960s. More scholarly than Howard Zinn's romanticized history, *SNCC: The New Abolitionists*, Carson's book is likely to be the definitive study of SNCC's rise and fall for a long time to come.

The book's strong points are many. It is based on extensive archival and interview materials. It is always a restrained critique, never a panegyric. It includes an excellent index. And, most important, Carson avoids all the excesses of much recent writing on the civil rights movement: pugnacious arrogance, uncharitable regard for previous scholarship on the subject, and promiscuous use of straw men.

On the negative side, the book lacks a bibliography. The notes, unhappily, are placed at the end of the book. There is little attempt to place the history of SNCC into a broader comparative context of civil disobedience. And, what is more surprising, the important concepts of "massive resistance" and "equality" are not even listed in the index, though they are touched on in the text.

Since SNCC's destiny was in a way a re-enactment of the abolitionist movement, the first Reconstruction, and the numerous antebellum slave revolts, Carson might have noted, but did not, that SNCC's history confirms once again the inexplicable way human beings (and their cultures) everywhere re-enact their tragedies. Perhaps in the final analysis the many human imperfections of SNCC and its elites simply authenticate the sincerity of the whole. Carson at least seems to suggest as much.

All told, this is a work of the most praiseworthy scholarship. Students of social protest will be deeply in the author's debt for years to come.

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## CANADA

V. A. TISHKOV. *Osvoboditel'noe dvizhenie v kolonial'noi Kanade* [The Liberation Movement in Colonial Canada]. Moscow: Nauka. 1978. Pp. 384. 2 r. 40 k.

Valerii Aleksandrovich Tishkov, an Americanist in the Academy of Sciences' Institute of General History, is the premier Soviet specialist in pre-1867 Canadian history. He has written several articles on the Canadian rebellion of 1837-38, and a large portion of the present work is devoted to that episode and the events leading up to it. His book as a whole treats the period in Canadian history from the British conquest to the early 1840s. In many respects, it is an expanded version of the same author's *Strana klenovogo lista: Nachalo istorii* [The Land of the Maple

Leaf: Its Early History] (1977), and as such reflects a common Soviet practice of obtaining the maximum possible mileage from a given topic of historical research.

By Soviet standards, Tishkov is a careful, even meticulous historian. He has worked with relevant materials in the Public Archives of Canada, the Public Archives of Ontario, and the Archive of Russian Foreign Policy (AVPR), besides consulting a number of documentary collections dealing with his subject. His familiarity with secondary works on this period of Canadian history is more than adequately demonstrated. If there is a weakness in the basic research for this volume, it is one of narrowness. Tishkov apparently has not investigated the holdings of the British Public Records Office and the U.S. National Archives on Anglo-American relations during this period, thereby neglecting two potentially rich sources of information.

Two flaws mar what is otherwise a good account of the events in question. One is an overemphasis upon the socioeconomic preconditions for the rebellion, a common failing in Soviet historical interpretations. The other is a seeming inability to place Canadian events in perspective with what was happening in Western Europe and the United States in these years. The movement to democratize the provincial governments of Lower and Upper Canada roughly parallels the Jacksonian era in the United States. The controversy surrounding the Reform Bill of 1832 in England is also a factor to be considered in evaluating subsequent developments in Canada.

Since most Soviet historical accounts of events in other countries have some degree of propaganda potential, the ulterior motives behind the publication of this and Tishkov's earlier work should be set forth. French-Canadian dissatisfaction with British colonial rule played a part in the 1837-38 rebellion. Coincidentally, René Lévesque's Quebec separatists had shown surprising strength at the polls shortly before Nauka Press developed an interest in early Canadian history. In 1838 the U.S. government was accused of violating its neutrality laws in behalf of the Canadian rebels. Not surprisingly, the extent of U.S. influence in Canada has been a matter of concern to Canadian authorities since the days of the American Revolution. Tishkov's book is a work of sound scholarship, but the Soviet Ministry of Foreign Affairs is also willing to take all the help it can get.

J. DANE HARTGROVE  
National Archives

CECIL J. HOUSTON and WILLIAM J. SMYTH. *The Sash Canada Wore: A Historical Geography of the Orange Order in*

Canada. Buffalo, N.Y.: University of Toronto Press. 1980. Pp. xii, 215. \$15.00.

Cecil J. Houston and William J. Smyth succeed admirably in their attempt to produce a quantitative study of Canadian Orangeism, measuring its rise and decline while sketching in social background. This volume, richly endowed with maps, charts, and graphs, provides a well-organized source of statistical material that all concerned with nineteenth- and twentieth-century Canadian history will find very useful.

Although Houston and Smyth do not offer a history of the Orange movement, they tell the story of its origin and growth in a manner that will hold the attention of general readers. Everyone who has studied Orangeism has noted its plebeian origins and democratic structure. Yet the myth that it was a Tory organization, created and maintained by vested interests, is still offered as fact not only in popular histories but also in academic studies as well. The tables in this book should remove any lingering doubts about the plebeian character of the movement and the ability of a religious and monarchist fraternal society to attract and hold a working-class following. For Orangemen "traditions chains" were the means of binding militia officers, professional men, politicians, and the occasional capitalist to a popular movement.

Orange rituals, picnics, and parades are placed in their geographical and social setting and against various backgrounds—pioneer settlements, fishing villages, mining- and milltowns, and large metropolitan centers like Toronto. In strange and often drab environments, the Orange tradition provided links with history and the outside world that in other circumstances have been offered by Roman Catholic and socialist movements. It also offered sickness and death benefits and the mutual aid provided by most fraternal societies.

In this account the statistical and social aspects of Canadian Orangeism are presented clearly, but the chapter on political and ideological dimensions leans a little on the authorized liberal interpretation of Orangeism. Orange tradition had its roots in seventeenth-century Whiggism; it is not clear that the tradition ever reached the proportions of an ideology. Liberals like Goldwin Smith appealed to the anticlerical instincts of Orangemen, imperialists to their belief in British institutions, and Tories to their fear of a government dominated by hostile radicals. In some instances these appeals could produce a temporary bloc vote, but in most elections Orangemen were divided and their Toryism much exaggerated by liberals seeking Catholic votes.

In considering the admirable charts and graphs, it is useful to remember Mark Twain's comments on statistics. Formal membership reflected the state

of Orange organization rather than its influence. Much of this influence was a result of Orange personalities like Ogle Gowan and John Hillyard Cameron. The absence of such personalities had much to do with the decline in Orange influence in the twentieth century.

The authors do not pretend to tell the whole story of Canadian Orangeism, but they have demonstrated the usefulness of the geographical approach to history. Efforts to weave the abundant statistical material they collected into a conventional history could have resulted in a long and tedious book. As it is, they have produced a short, useful, and readable volume.

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## LATIN AMERICA

JORGE I. DOMÍNGUEZ. *Insurrection or Loyalty: The Breakdown of the Spanish American Empire*. (Publications of the Center for International Affairs.) Cambridge: Harvard University Press. 1980. Pp. ix, 307. \$29.50.

This is an initially baffling book—baffling, upon reflection, for two major reasons. First, it proceeds on two tracks. The main one follows Jorge I. Domínguez's stated procedure of looking at selected hypotheses of political science in relation to historical example. The other track, only intermittently discernible, snakes from passing mention to incidental observation and provides, in passing, glimpses of the truly Laocoön nature of the causes of breakdown of Spain's American empire. The second major problem is the choice and handling of theme. The author addresses the question of insurrection or loyalty, which asks why wars of independence did not occur everywhere in Spanish America, by identifying decisive factors differentiating loyal from insurgent colonies, and these factors come down to "the political relations between elites participating in politics and the imperial and local governments responding to them" (p. 2). Thus principal hypothesis, theme, and conclusion are one, and the body of the book is designed to defend it, or them, by taking up and dismissing other possibilities as insufficient or faulty explanations.

Four Spanish colonies—Chile, Cuba, Mexico, and Venezuela—are discussed from the late eighteenth century through independence. Although the areas chosen are all nations today, they were less comparable as imperial units. Chile, for example, was more analogous to certain regions within Mexico. The book discards more general explanations for insurrection, such as social mobilization, the political participation of the elites, the development of



a sense of national consciousness, and issues of foreign trade. It also rejects the competition between Spaniards and Americans, the relative deprivation of individuals and groups, and the impact of new European ideas. Wars of independence, we are told, were not extensions of colonial revolts. Although it is true that these factors do not suffice as general explanations in themselves and that they were present in Cuba, which remained loyal, nevertheless the network relationship among them all is the key to the independence period. Political stance and bargaining were a part of this web; they did not have a life of their own and were often subsidiary to other elements. Thus Cuba was an exception not simply because its domestic political relations and those with Spain differed. Rather, they differed because of a host of highly significant other reasons (many of them mentioned in what I have called the book's second track). We are directed not to root causes here, but to common denominators.

Although Domínguez provides some worthwhile insights into what happened among interest groups in Latin America—especially from 1810 to 1824—the book is not reliable on the evolution of Spanish imperial theory and policy or on their American reception. It does not give due weight to the pull of regionalism away from viceregal centers. It leaves out the complex influences and combined impact of concepts of political economy, constitutionalism, and the Enlightenment. There is no mention of the crucial interplay among altered Spanish policies, foreign trade, and American economic conditions and attitudes in the first decade of the nineteenth century. How can 1808 be discussed without reference to Spain's blatant milking of the colonies, especially through the vast, interlocking projects centered in the *Caja de Consolidación*, the government agency functioning as a kind of central bank, and having multinational ramifications? Recent, indispensable findings by Jacques Barbier, Harold Bierck, Humberto Tandrón, and John Jackson on this subject should, but do not, appear among the large number of sources cited. We have here, then, a very narrow book, of interest to specialists in the independence era and to some political scientists. Historians need, for the better understanding of Latin American insurrection and loyalty, studies like those done in United States history by, among others, Ian Christie, J. G. A. Pocock, J. R. Pole, and Gary Nash.

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URS HÖNER. *Die Versklavung der brasilianischen Indianer: Der Arbeitsmarkt in portugiesisch Amerika im XVI. Jahr-*

*hundert.* (Beiträge zur Kolonial- und Überseege-schichte, number 18.) Zurich: Atlantis Verlag. 1980. Pp. 272.

An essential part of the process of acculturation in the New World was the diversity and intensity of attempts by Europeans to utilize American Indians as sources for labor. The question of Indian labor in sixteenth-century Brazil is the object of this study by the Swiss scholar Urs Höner. Opening with the obligatory ethnographic survey, Höner goes on to describe the cordial relations between Portuguese and Indians in the period between 1500 and the establishment of donatary captaincies. In exchange for tools and trinkets, Indians provided labor for the dyewood industry. Purely commercial interests were to give way to socioeconomic imperatives. European perceptions of the indigenous population changed. Apprehensive of French rivalries and moved to compensate for a declining *Estado da Índia*, the Portuguese colonized and established a seat of royal government in Brazil. Sugar provided the foundation for an export-oriented economy, and all aspects of the industry are here described at length. But the most important section (and that which provides the subtitle) comprises seventy pages on the labor force required for the expansion of the industry and on those methods (*resgate, entradas*) and justification (*guerra justa, extrema necessidade*) for "recruiting" Indian labor. The author concludes with a description of the rivalries and hostilities between colonists and Jesuits, each seeking to control the labor system; the importance of the *aldeias*; and an overview of Jesuit policies and practices.

This is well-trodden ground. Apparently Höner was unaware of John Hemming's *Red Gold* (1978) which, albeit diffusely, covers the same issues. No less surprising is the omission of reference to Alexander Marchant on the transition from barter to slavery or to Dauril Alden's invaluable study on the struggle for "Freedom of the Indians." Höner's sources are primarily travelers' narratives, Jesuit letters, and standard histories of the period; the author makes only limited use of archival and library resources in Portugal. The interest of this volume lies not in the sources, however, but in the interpretation. Höner challenges two major assumptions: that the Portuguese crown, recognizing its responsibility to the Indians, drew up legislation intended to prohibit their enslavement and that the Jesuits established *aldeias* independent of official intervention and sought to protect their charges from exploitation. He would argue that as early as the 1530s the crown cast a blind eye on evidence of forceful recruitment of Indians. It was the violence of Tupi reaction and a generally chaotic situation concerning the use and procurement of Indian la-

bor that led, in part, to the crown's decision to appoint a governor general and provide Tomé de Sousa with a *regimento* laying down criteria for selection of, and guidelines for, such recruitment. Portuguese policy was dictated by two considerations only: consolidation and strengthening of imperial control and the maximum development of the sugar industry. Höner maintains that at no time during the pursuit of these objectives, was the use of Indian labor in the sugar industry questioned. He also argues that in implementing its policies the crown essentially used the Society of Jesus. It was to the Jesuits that the crown turned to bring order to hitherto uncontrolled attempts at enslavement and to the capricious justifications for such acts. More stringent guidelines were not intended to diminish, but to increase, the numbers of Indians recruited. Höner insists that the interests of the Jesuits far exceeded their primary missions to convert and catechize: the society and individuals such as Manoel da Nóbrega were deeply concerned about the colonization and administration of Portuguese America. Following this line of thought, the author suggests that the *aldeias* were less the fruits of missionary zeal and recognition of the humanity of the Indians than a decision by the Society to further crown policy, which looked on the *aldeias* as a labor pool for the sugar industry. References to the "ghetto atmosphere" of the *aldeias*, the disruption of mores and values, and the charge that the Jesuits destroyed the bases of existence itself for the Indians complete a harsh assessment of the role of the society.

*Die Versklavung* adds heat but casts little light on the questions surrounding Indian labor in Brazil. There is an unwillingness on the part of the author to acknowledge the existence of alternatives to the system he describes. And yet, with an admittedly checkered record of success and depending on time and place, there were alternatives to chattel slavery. Two such forms were the attempted creation of an indigenous peasantry and the slow—if not very successful—integration of Indians into the capitalistic market as wage earners. A variety of arrangements governing the use of such labor included barter, enslavement, and payment of wages. Finally, although the Indian contribution to the building of sugar mills and the consolidation of the industry in the 1550s and 1560s cannot be denied, already by 1570 a reduction in availability and profitability of Indian labor led plantation owners to turn to West Africa as a source of labor. This volume neither supersedes Georg Thomas's *Die portugiesische Indianerpolitik* (1968) nor provides those insights contained in Louis Necker's *Indiens Guarani et chamanes franciscains* (1979). For the English reader the essays of Dauril Alden and Stuart Schwartz remain the most effective syntheses of the problems of reconcil-

ing Portuguese crown policies with competing New World demands for labor.

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JORGE CASTELLANOS. *La Abolición de la esclavitud en Popayán, 1832-1852*. Cali, Colombia: Departamento de Publicaciones, Universidad del Valle. 1980. Pp. 132.

In this slim volume Jorge Castellanos, a Cuban-born historian who teaches in Detroit, presents an admirably lucid account of the decline and extinction of slavery in one of the few provinces of New Granada where the institution was truly significant, politically as well as economically. It is clearly organized and written, is based on careful research in the Archivo Central del Cauca and elsewhere, and is well balanced—almost to a fault—in matters of interpretation.

Although the author introduces many new facts, the main lines of the story he tells are familiar to students of Colombian history and in particular to anyone who has followed the work of J. León Helguera (a name that is inevitably cited with some frequency). After first sketching the physical and social environment of the Cauca region and the Gran Colombian antecedents, Castellanos describes in rather more detail what happened in the province of Popayán once New Granada became a separate republic. He points out the glaring deficiencies of practical implementation of the Gran Colombian manumission law of 1821; the contribution of racial tensions to regional civil strife and of civil strife, in turn, to the further weakening of slavery; and the final abolition that came as one aspect of the mid-century heyday of liberalism.

In addition to presenting mere facts, the volume touches on a broad range of significant and often controversial questions, including the relative mildness or harshness of the actual treatment of slaves, the relation—whether cause or effect—between the decline of slavery and the economic decline of Popayán and its dependencies, and the extent to which emancipation really made a difference in the lives of the slaves themselves. In dealing with these questions, Castellanos never fails to mention contrasting views or evidence, but he does not often attempt to settle them. Obviously, no definitive answers could have been formulated in so brief a space, particularly since the author shows a no less obvious caution in making judgments. The result, in any event, is that this case study excels more as the description of a process than as a medium for testing or transmitting new ideas and insights.

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LAWRENCE A. CLAYTON. *Caulkers and Carpenters in a New World: The Shipyards of Colonial Guayaquil*. (Papers in International Studies, Latin America Series, number 8.) Athens: Center for International Studies, Ohio University. 1980. Pp. ix, 189.

With notable and (in this brief review) nameless exceptions, decades passed before historians of colonial Spanish America began to realize that the territories conquered and settled by Spaniards in the New World quickly became semi-independent socioeconomic units. There were, to be sure, ties with the mother country that endured for centuries, but subservience was a fiction that even the lawbooks belied. One has merely to read the works of James Lockhart on Peru, John Phelan on Ecuador, and Peter Bakewell on Mexico to grasp this point. In Lawrence A. Clayton's well-researched and clearly written account of the shipyards of Guayaquil, still further evidence of interdependence is forthcoming.

The spectacular feats of Magellan and Drake demonstrated that navigation around the Horn to the Pacific Coast was possible but hazardous, and, until the late seventeenth century, Spanish shipyards, for all their vaunted reputation, did not possess the requisite technology in ship design to make routine voyages by this channel a feasible proposition. Luckily for Spain, the woods and workmen of Ecuador were available to build the military and commercial vessels to protect and maintain contact with the Pacific Coast via the Isthmus of Panama. Clayton studies in detail the building of these ships and in the process makes an important contribution to our knowledge of Guayaquil and of the economic development of Ecuador.

What is of particular interest is the author's description of the interaction between royal concerns—dictated by European pressures scarcely perceived by the Ecuadorians except when they were attacked by Drake or the Dutch—and the realities of shipbuilding on a Pacific coast never seen by a Spanish king. In miniature form, we have here yet another description of a colonial industry ruined in the eighteenth and nineteenth centuries, much like the manufacture of Ecuadorian textiles, by European technology. With better sailing ships to carry superior goods from Europe around the Horn, the need for Ecuadorian shipbuilding and the importance of Panama as a commercial center vanished. I highly recommend the book to those who wish to understand the balance among European colonialism, its effects on a given region, and technological change during the dawn of what is perhaps imprecisely termed the modern age.

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HERMAN W. KONRAD. *A Jesuit Hacienda in Colonial Mexico: Santa Lucia, 1576–1767*. Stanford: Stanford University Press. 1980. Pp. xii, 455. \$28.50.

Beginning with Francois Chevalier's chapter on "The Great Church Estates" in *Land and Society in Colonial Mexico: The Great Hacienda* (1963) and followed by the dissertation of James D. Riley on the management of Jesuit estates in 1971, a body of modern literature on Jesuit haciendas in Mexico has emerged. Herman W. Konrad's in-depth study of Hacienda Santa Lucia is a welcome addition to the field. It makes extensive use of archival material in Mexico City and in repositories in the United States, but it does not include manuscript sources from Rome, Spain, or the restricted papers of the Jesuit province in Mexico that can be consulted given the proper entree. To a large extent Konrad traverses the same body of manuscripts used in Riley's 1971 dissertation and in that author's 1976 book, and for the most part his conclusions parallel Riley's. Riley did publish in 1973 a study on Santa Lucia in the eighteenth century that Konrad has not used. The book-length article of Felix Zubilliga, "La provincia Jesuitica de Nueva Espana: Su fundamento economico: Siglo XVI," (*Archivum Historicum Societatis Iesu*, 38 [1969]: pp. 3–169) was not consulted by the author and it does cover material included in two chapters of his book. More effective use of Riley's work and the Zubilliga item might have led Konrad to other manuscript data, thereby adding detail and clarity to Santa Lucia's history.

These gaps in research aside, this is a very good study. Konrad brings to his task the perspectives of both anthropology and history; hence, his analyses are fresh and valuable to scholars in both fields.

The study is divided into four parts, including impressive chapters on the historical background of the Jesuits' construction of their corporate empire in Mexico. A second section gives important data on the structural aspects of management of Santa Lucia and how the hacienda functioned. In these chapters Konrad has a difference of opinion with Riley regarding the nature of control that the Jesuits exercised over their labor force. One chapter especially is outstanding—"Days, Seasons, and Life-Styles" goes far beyond any interpretive materials we have had on hacienda life. A final section deals with the external relations of Hacienda Santa Lucia and tries to relate the institution to the broader context of Mexican politics and economy in comparative perspective. Although Konrad does a good job in this section, he might have also used John M. Tutino's dissertation, rather than his 1975 article, to show comparative data on linkages between the economy of Mexico City and the haciendas and Indian villages of the northern part of the valley.

Konrad's articulate and erudite conclusions draw heavily on his knowledge of anthropology and ethnohistory and make this book especially valuable.

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MARK A. BURKHOLDER. *Politics of a Colonial Career: José Baquijano and the Audiencia of Lima*. Albuquerque: University of New Mexico Press. 1980. Pp. xi, 184. \$20.00.

In an earlier study of royal appointment policy to the American *audiencias* between 1687 and 1808, Mark A. Burkholder and his coauthor D. S. Chandler demonstrated that over half of all the appointments made to the high courts during the period 1687–1750 were awarded to Peruvian Creoles, many of them native sons who had been born and raised in the districts where they had been appointed. After mid-century, however, Charles III and his successors radically altered these policies and procedures in an effort to regulate the number of Creole appointees to the courts. This book traces the political career of one of these ambitious and frustrated *pretendientes*, the Peruvian nobleman José Baquijano y Carrillo, which culminated with his appointment as a criminal judge in Lima in 1797, the first native son to have been directly appointed to the court in the space of two decades. In this sense, it gives a regional and personal dimension to a subject treated by the author in his earlier work.

The Peruvian elite, of which Baquijano was a respectable but not an outstanding member, was a profoundly insecure and fearful group, threatened on the one hand by ambitious Spaniards and on the other by numerous *gente decente* and unwashed masses. Baquijano's life provides ample testament to this fearful and excessively ambitious state of mind. Although socially and economically advantaged, owing to complex factors of family wealth and background, Baquijano encountered enormous obstacles on his tortuous path to governmental office. In 1776 he was unceremoniously expelled from Spain by Charles III for profligacy but recovered sufficiently in Peru to receive the first chair of civil law at San Marcos, to attain the presidency of the Society of Friends of the Country, and to contribute to the journal *Mercurio peruano*. In between, he escaped censure for his ill-advised eulogy of the discredited Viceroy Agustín de Jáuregui. Following his appointment to the Lima court in 1797, Baquijano earned membership on the short-lived Council of State before that body was abolished by the returned Ferdinand VII in 1814. He died in Seville in 1817 still seeking further political honors that would

allow yet another triumphal return to his native Lima.

By tracing a single political career against a backdrop of changing appointment policy, Burkholder certifies the insatiable quest of American Creoles for prominence through high bureaucratic posts and the rationale that governed appointments. The author makes no effort to mask the frailties of his subject—who clearly cared more about advancing his career than about the interests of his constituents or country—and in so doing revises the views of certain Peruvian historians who depict Baquijano as a true son of the Enlightenment and a precursor of independence. Burkholder contends, however, that Baquijano's appointment was based on merit and that to his peers he was a good *oidor*, a judgment made despite the shadowy presence of an enormously wealthy and well-connected brother in Spain.

The book is an exceptionally well-researched and generally well-written study of Creole aspirations for office and status. Burkholder demonstrates that the data advanced in his earlier work are valid at the regional level in an exquisitely detailed monograph that will be read with particular interest by students of Bourbon reformism. Clearly Baquijano's aspirations to a career in government can stand for a generation of Creole aspirants to office. The book's appeal to a general audience might have been improved, however, had Burkholder placed Baquijano's career within a more theoretical perspective similar to that developed in John Phelan's *Kingdom of Quito*. By focusing on the issue of the bureaucratization of power through Baquijano's attitudes and performance in office after 1797, the author could better address the important question of how changing ethnic composition patterns affected the quality and tenor of colonial government. Similarly, although changing appointment policy after 1750 clearly exacerbated Creole-peninsular tensions, this is a venerable phenomenon of complex origin that deserves fuller study. These criticisms aside, however, *Politics of a Colonial Career*, by focusing on the life of one important Peruvian office seeker, constitutes a genuine contribution to the history of administrative politics in late colonial Spanish America.

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CHARLES R. BERRY. *The Reform in Oaxaca, 1856–76: A Microhistory of the Liberal Revolution*. Lincoln: University of Nebraska Press. 1981. Pp. xviii, 282. \$20.00.

Charles R. Berry's book on the reform in Oaxaca is one of several excellent contributions to the litera-



ture of a critical period in Mexican history that have been made during the past decade. These recent scholarly efforts, most of which are based on extensive archival research, bring to the topic a more sophisticated evaluation of the liberals who made the reform than many earlier studies and enhance our understanding of the emergence of modern Mexico. Berry and others, although often viewing the liberals with considerable sympathy, nonetheless have redrawn their portraits from life. They have thus revised an era and a group that were badly in need of revision. No longer clad in the mythic robes of populism, altruism, and heroism, the Mexican liberals can at last be seen as they really were: a largely self-serving group of ambitious, middle-class politicians preoccupied with power and personal advancement, insensitive to the overwhelming majority of the Mexican people, and not especially competent as national or even local leaders.

Berry convincingly makes several important points about the reform in Oaxaca and throughout Mexico. Factionalism, a consistent problem for the liberals and a major obstacle to implementation of their modernizing program and to civil peace when they were in power without any active opposition, was especially intense in Oaxaca. Partly this stemmed from the persistent strength of the moderate liberals, who balked at the extreme anticlericalism of the so-called radical faction and had enough political vitality to wrest control of Oaxaca from the radicals on several occasions. Factionalism also raged when two Oaxacans, Benito Juárez and Porfirio Díaz, twice contested the presidency. The political behavior of the liberals during these power struggles, as described in detail by Berry, is far from edifying.

Berry demonstrates that in Oaxaca there were several reforms, not just one, for liberal changes often did not reach beyond the Central Valley region, and the nature of the reform program itself changed, depending on the faction in power in the state. Berry also shows that there were several reforms throughout all of Mexico. In contrast to what occurred in the central region of the country, in Oaxaca disentanglement and alienation of church property by liberal fiat did not involve purchases by large numbers of merchants and foreigners, nor were such acquisitions made for speculative purposes. Also in contrast to central Mexico, where liberal land legislation brought havoc to Indian communities, Oaxacan reformers had little success outside the Central Valley region (where there were few Indians) in implementing their agrarian policy. Thus, with the exception of the Mixtecs and some other peasant peoples in isolated areas who heartily detested the liberals, the reform in Oaxaca did not meet with as much resistance among the rural population as it did elsewhere.

Concluding his fine study—parts of which without malicious intent cast the liberals in a disreputable light—Berry wonders if changes wrought by the reform were worth their heavy price in suffering, socioeconomic dislocation, and political turmoil. They might well have been, but since the liberals themselves stimulated rather than curbed military praetorianism, as Berry pointedly notes, they also led logically to the long Díaz dictatorship that followed and culminated not in the successful modernization of Mexico but in the bloody revolution of 1910.

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FERNANDO URICOECHEA. *The Patrimonial Foundation of the Brazilian Bureaucratic State*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xvii, 233. \$16.50.

This work is a valiant intellectual effort by a Brazilian scholar to probe historico-sociopolitical underpinnings of the emergent bureaucratic state during the course of the nineteenth-century Brazilian empire. The model of analysis is the Weberian ideal of patrimonialism, against which the author measures his evidence, finding that slave-holding planters on their latifundia created a sociopolitical phenomenon in which "the patriarchalism of local society corresponded to the patrimonialism of the larger society" (p. 19). Brazilian titled nobility—many of whom were landlords—did not, however, as a class enjoy aristocratic privileges, since "no politically guaranteed status structure of nobles existed" (p. 25).

Fernando Uricoechea makes clear that the values of Portuguese *fidalgia* (nobility of the blood) could not have developed in this rural-based society because it was rooted in a military ethos that was lacking in the relatively peaceful milieu of Brazilian agrarian society. According to the author, political sociologist Oliveira Vianna's intuition about the importance of the Brazilian clan complex was "sociologically correct but improperly conceptualized," because that configuration conformed more closely to the concept of tribe. In a word, behind the Brazilian "clan" there was the landlord's family, over which the authority-figure of the *pater familias* ruled; hence, a fierce individualist as opposed to collectivist pattern of authority resulted.

The author traces the evolution of the Brazilian bureaucratic state during the nineteenth century primarily through the institution of the *Guarda Nacional*, or provincial militias, whose origin dated from the second quarter of the century and whose purpose it was to systematize and institutionalize the authority of the prince and the state through

mobilization of corporate units composed of free-men and agrarian notables at the local level.

Through painstaking archival research involving massive quantities of official correspondence at local, state, and national levels, Uriceochea concludes that it was only in the southern frontier province of Rio Grande do Sul—due to the foreign military threat from Argentina—that the Weberian rational ideal of the bureaucratic state was partially achieved. Elsewhere, “the institutionalization of a legal order, bureaucratically administered by the state,” often broke down, especially in the backlands. The poor performance of the Brazilian state in pursuing the Paraguayan War (1865–70) to a successful conclusion accelerated an already evident trend toward a more rational, merchant-dominated state and society by the end of the century.

Uriceochea maintains that the *Guarda Nacional*, despite its shortcomings, contributed to the “creation of the modern bureaucratic state in Brazil” (p. 183). This was because it provided continuity of public administration in the process of state building. One is left, however, with an uneasy feeling that perhaps the case here is a bit overdrawn, in spite of a supreme heuristic labor to give shape to a massive body of evidence that as yet seems to defy precise analysis and classification.

Even so, it is highly laudable that a talented researcher has done noble battle with the amorphous bureaucratic “creature” of the nineteenth-century Brazilian state in an attempt to inject some sophisticated rationality into a subject notable for its neglect—with few exceptions—by scholars. One hopes that this pioneer study will be a precursor of many to follow.

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DONALD L. HERMAN. *Christian Democracy in Venezuela*. Chapel Hill: University of North Carolina Press. 1980. Pp. xiv, 289. \$24.00.

Christian Democracy has been more politically successful in Venezuela than in any other Latin American country. Yet surprisingly little has been written in English on the movement that has won two of the last three presidential elections. Donald L. Herman's study is thus an important addition to the meager literature on this topic. The book has two major sections. The first provides a comprehensive historical account of the origins and development of the Christian Democratic movement in Venezuela. It covers thoroughly the ideological evolution of the movement and is particularly enlightening on the powerful intellectual influence that the famous Jesuit Father Manuel Aguirre had on Rafael Caldera and other members of the Christian Demo-

cratic founding generation of 1936. Also informative is the author's account of the complex generational and ideological divisions within the Social Christian COPEI party. The final chapter in this section describes in detail the structure and function of the party and its affiliate organizations.

The second section studies the policies and performance of the first Christian Democratic regime to come to power in Venezuela, that of President Rafael Caldera (1969–74). Herman, who is generally evenhanded in his assessment of the successes and failures of the first COPEI government, suggests that Caldera was more successful and innovative in his foreign policy, particularly as a champion of “international social justice,” than in his domestic policies. COPEI's 1968 campaign slogan, “El Cambio,” was not fulfilled in the sense that the Caldera government brought about deep structural changes in Venezuelan society. Indeed, Herman stresses the essential continuity between the Caldera regime and previous Acción Democrática governments and underscores the importance of AD-COPEI cooperation in preserving the post-1958 democratic system. He concludes that the Christian Democrats, despite having moved toward the left since the 1950s, are not committed to bringing about genuinely radical change. A powerful faction within the party as well as COPEI-oriented business supporters are too conservative to permit the movement to abandon its moderate reformist stance.

*Christian Democracy in Venezuela* is a solid, workmanlike study through which students of modern Venezuela can reach a better understanding of the reasons for COPEI's relative success and the nature of its contributions to national political development. It is surprising, however, that a political science work such as this makes little use of political theory and fails to fit the subject matter into some systematic analytical framework. Also, the author's style of writing is pedestrian and almost casual in places, and the narrative flow is interrupted by too many subsections within each chapter. There are, in addition, too many loose statements such as “no government can solve the problem of poor labor efficiency and lack of a work ethic” (p. 164). Finally, although he makes extensive use of interviews with public figures from Caldera to local party functionaries, Herman tends to accept their statements at face value. But despite these limitations, this book stands as the most comprehensive and objective study yet on one of the two major parties in Latin America's most prosperous democracy.

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THOMAS H. HOLLOWAY. *Immigrants on the Land: Coffee and Society in São Paulo, 1886–1934*. Chapel Hill: Univer-

sity of North Carolina Press. 1980. Pp. xviii, 218. \$21.00.

MARCO PALACIOS. *Coffee in Colombia, 1850-1970: An Economic, Social, and Political History*. (Cambridge Latin American Studies, number 36.) New York: Cambridge University Press. 1980. Pp. xv, 338. \$34.50.

In the last decades of the nineteenth century and the first decades of the twentieth, coffee was one of the most important items in the rapidly expanding system of world trade. Just as increased coffee consumption reflected deep changes in cultural patterns and social organization in the rich industrialized nations of the North Atlantic, coffee production transformed the face of whole societies in the tropics. The two books under review broaden our understanding of central features of that process in the two most important coffee-producing nations of Latin America: Brazil, whose giant's share of the fast-growing world coffee market expanded from about half in 1850 to three-fourths by the turn of the century, and Colombia, where production developed dramatically after 1890 to capture one-fifth of a total world market still dominated by Brazil in the 1940s.

In the last years of the nineteenth century, Brazilian coffee production shifted from slave to free labor by attracting tens of thousands of European immigrants, especially Italians, into coffee cultivation. The large Brazilian planters who dominated production in the major coffee-producing state of São Paulo accomplished this goal through two means, one voluntary, the other forced on them by their workers. They mounted an aggressive state-funded recruitment program in Europe that paid transportation and initial location expenses for migrant families. And they developed labor and land tenancy systems that made work in coffee production tolerable enough to attract and retain free European labor. Thomas H. Holloway explores the genesis, evolution, and social implications of each strategy in his disciplined, detailed, and persuasive book. Immigrant coffee families (*colonos*) received wages, housing, and access to the land to grow food crops on terms that made saving and social mobility possible. By the 1930s, contrary to assumptions widespread in the literature, small and medium farms (roughly twelve to sixty hectares), the majority owned by foreigners, accounted for more than half of the coffee farms in São Paulo and contained about a fourth of producing trees. Although Holloway cannot positively confirm that most of these foreign small and medium landholders were former *colonos*, he marshals an impressive range of qualitative and quantitative evidence to argue that they probably were. This single-minded monograph does

not address the broader economic and political implications of its startling analysis of worker mobility and landowning structure. Questions of this sort are more frequently asked in the Colombian literature.

As Colombian coffee cultivation expanded from the northeast to the south and west over the course of the last century, small units (much smaller than Holloway's "small" Brazilian farms) grew in number and importance. Coffee production in Colombia tapped labor reserves within the country through a bewildering range of labor and land tenure systems, all of which tended to blur the neat capitalist dichotomy of ownership and wage labor. For example, *estancieros* in the southeast received land in exchange for labor obligations. But these obligations might be satisfied by hiring substitutes paid with profits realized in the sale of agricultural commodities, even coffee, produced on the land extended *estancieros* by large landowners. The social and ideological result of land and labor arrangements like these was the making of a rural working class in the coffee zones of Colombia torn between the individualist aspirations of petty capitalists and collective struggle for social reform. Most of the time, in most places, individualist capitalist aspirations and values prevailed. Growing social tensions were channeled through traditional legal and political means to yield a pervasive, often violent struggle for control and access to the land. Meanwhile, limited in their ability to appropriate surplus directly through absolute control of the means of production, domestic and foreign capitalists worked (increasingly with the help of the state) to expand their control of coffee processing and commerce.

Several aspects of this process, which is the focus of some of the best recent Colombian scholarship, are elucidated in Marco Palacios's book. The subject is vast, as indicated in the book's subtitle. Rather than write such an ambitious history, Palacios contributes to it very suggestive, if somewhat unconnected and undeveloped, snapshots of several of its dimensions. He advances comparisons of regional modes of coffee production that merit detailed investigation, comparative explanation, and evaluation in social and political terms. He challenges the connection between small coffee farmers and industrialization that informs some of the best Colombian economic history. He shades the "white legend" of coffee colonization in the west with black tones of commercial exploitation and endemic private and public violence. Much of the struggle for land on the coffee frontier was expressed in land law and court cases. Palacios, initially trained as a lawyer, moves through this material with authority. More than any previous investigator, Palacios has uncovered and demonstrated the utility of a range of previously neglected sources—most notably the

archives of commercial firms—that will provide information to advance all these debates.

These important works remind us how rudimentary is our knowledge of the pervasive influence of coffee export economies on national development in Brazil and Colombia. They uncover sources, de-

velop research strategies, and raise comparative questions that will enable Latin American historians to tackle this vital research problem with renewed vigor.

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## Collected Essays

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These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

PAUL BAIROCH and MAURICE LÉVY-LEBOYER, editors. *Disparities in Economic Development since the Industrial Revolution*. New York: St. Martin's Press. 1981. Pp. xviii, 428. \$37.50.

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# Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."*

TO THE EDITOR:

Richard L. McCormick's "The Discovery That Business Corrupts Politics: A Reappraisal of the Origins of Progressivism" (*AHR*, 86 [1981]: 247-74) makes an impressive argument. The author's reasoning suffers, however, on page 257, where he referred to realignment elections four times, apparently unaware of Allen J. Lichtman's "Critical Election Theory and the Reality of American Presidential Politics, 1916-1940" (*AHR*, 79 [1976]: 317-51).

RAYMOND J. JIRAN  
*Thomas Nelson Community College*

TO THE EDITOR:

Jack M. Patt's review of my book, *Conradi und Hamburg: Die Anfänge der deutschen Adventgemeinde (1889-1914) unter besonderer Berücksichtigung der organisatorischen, finanziellen, and sozialen Aspekte* (*AHR*, 85 [1980]: 150-51) contains errors and misleading statements. The major ones I would like to correct.

Professor Patt stated that my book "is an incomplete account of Conradi and the Adventist Church in Germany." My title indicates clearly that this was never intended. It is an account of Conradi and the beginnings of the Adventist Church—covering twenty-five years only—in *one city*, which happened to play a significant role in the church's history in Germany. Patt accused me of speculating in my "charge that the European General Conference was dissolved because of Conradi's moral failure." This I did not "charge" at all. Rather, I stated that a budget deficit seems to have been the cause for the organizational change (p. 180).

Professor Patt claimed that my "research is suspect" when I stated that "no Adventist German bore arms before 1914." I stated, on the contrary, that in most cases it was not the bearing of arms but the service on Sabbath that brought Adventists into a conflict with their conscience (p. 242). (It looks like it is Patt's review that is suspect.) Patt accused me of ignoring "any discussion of the change of church policy concerning military service that took place once Germany was involved in World War I." This no doubt would be an interesting subject for research, but, as the title of my book indicates, I did not cover World War I at all.

Professor Patt concluded that my book "does not add significantly to historical knowledge." I believe the opposite, and I have at least two good reasons for that: (1) I found a large share of my sources in the Archives of the General Conference of Seventh-Day Adventists, archives that were not in existence before 1972; (2) I received some significant information from the files of the political police in Hamburg, files on the Adventist Church that were not used before by any researcher.

GERHARD PADDERTZ  
*Solusi College  
Bulawayo, Zimbabwe*

TO THE EDITOR:

It is not my belief that there is generally much to be gained in a scholarly sense in acrimonious exchanges between the reviewers of books and those subjected to critical appraisal of a negative kind. Ronald G. Walters's review of my book, *Sobering Up: From Temperance to Prohibition in Antebellum America* (*AHR*, 86 [1981]: 209), is useful, however, because it illustrates certain conceptual, methodological, and epistemological problems within the profession that must be confronted before further progress in the writing of social history can be achieved.

Let me begin, as Professor Walters did, with the least significant and most justifiable criticism. Walters noted the relative absence of discussion of the South, and he questioned whether this reflects the

social reality or organizational bias in the records. Although I did of course anticipate this criticism from a profession so empirically minded as mine by indicating the geographic scope of the study on pages 12–13, I probably also ought to have subtitled the book to refer only to the antebellum North. Yet my conviction that temperance was especially weak in the South has not been shaken either by my own current research into that subject or by Walters's "hunch" that I am wrong. For the moment, however, I must rest my case until the forthcoming appearance of my article "Drink and Temperance in the Antebellum South" in the *Journal of Southern History*.

Professor Walters went on to accuse me of terminological imprecision leading to a "blunt" and "rhetorical" analysis. Let me follow up in detail one example. He accused me of describing the temperance leadership as "upwardly mobile captains of industry." But he neglected to quote the earlier part of that sentence, which stated, "... the older evangelicals and wealthy captains of industry ..." (p. 167, italics added). He also neglected to mention the fact that I was providing a single sentence summary of those who "by and large" had led the pre-1840 temperance societies. This summary was provided, which Walters also failed to mention, in the context of great detail about the very different and newer Washingtonian and fraternal temperance societies of the 1840s. Walters's representation of my position here is a distortion quite unexpected from so subtle and skilled a historian as he undoubtedly is.

It also ought to be made clear, contrary to Professor Walters's claim, that I do not regard the 1850s Maine Law period as merely an extension of the pre-Washingtonian movement. Nor did I depict the Washingtonian phase as simply a "detour"; my approach was basically dialectical in which there occurred by the 1850s a synthesis in both tactics and support of the pre-1840 movement and elements within the artisan societies of the 1840–50 decade. Class lines, initially unclear in the early 1840s, became much more distinct as the antiliquor question shifted from temperance to Maine Law prohibition. It is prohibition, not temperance, that I regarded as a fundamentally middle-class movement in the early 1850s.

As for the extraordinary claim that my "heart" was "not in assessing the beliefs of temperance men and women," I will leave that for the determination of each and every reader, only cautioning that they should look for the very considerable discussion of both beliefs and actions not just in the one chapter Professor Walters singled out but in almost every chapter in the book. What Walters seems to have been looking for here is an analysis in which time is held largely constant and the structure of beliefs is examined. He chose to write such a book, *The*

*Antislavery Appeal* (1976), on a related reform movement. Although such an approach has value, it is my conviction that beliefs have to be related very closely to actions, and especially to questions of power and change over time. That is why I chose to integrate my discussion of beliefs with the political, social, and economic realities of the drink question in its various temporal phases.

I have left the most important point until last. Professor Walters has complained that my understanding of class is "remarkably loose." I shall not worry too much that he tried to refute my claims about "property and class"—a statement made with specific reference to the coercive Maine Law phase of statewide prohibition—with allegedly inconsistent evidence drawn from my study of the earlier no-license movement in Worcester of the 1830s. Let me instead concentrate on methodology and epistemology. A close reading of Walters's criticism suggests the problem lies less in my own view of class than in Walter's economic conception of that phenomenon. His own, apparently more precise, view equates class with indicators like "wealth" and separates the concept of class from culture and consciousness. It must be emphasized, as unsuspecting readers of the review might be misled, that this is Walters's economic conception of that phenomenon. His own, apparently more precise, view equation of class with (usually) quantifiable economic variables, which is a product of a static, social stratificationist viewpoint that still retains some force within the American historical tradition. Class, culture, and consciousness are not discrete categories that can be mechanistically separated for purposes of analysis and then put back together to show how they "interacted." Historians like Walters might better understand these points if they would take more seriously the critique of liberal-empiricist conceptions of class contained in James R. Green's "Behavioralism and Class Analysis: A Review Essay on Methodology and Ideology" (*Labor History*, 13 [1972]: 89–106). Such articles, however, can only be ineffectual protests against the academic tide until someone has completed a systematic analysis of the epistemological and methodological foundations of liberal history—a tradition within which, whether he accepts it or not, Ronald Walters is very firmly entrenched.

IAN TYRRELL

*University of New South Wales*

#### PROFESSOR WALTERS REPLIES:

In common with Ian Tyrrell, I want to see progress in the writing of social history. Neither progress nor methodology and epistemology, however, will gain from misrepresenting a reviewer's position. In two instances, Tyrrell deflects criticism by claiming to

know—and then disputing—my views. In each instance he is wrong.

Contrary to his assumption, I was not looking for a book like mine. I am not writing that kind of book now, and I do not expect it from others. I would like to have seen an analysis integrating beliefs and behavior. Dr. Tyrrell and I simply disagree on how thoroughly he did that.

Dr. Tyrrell also claims to know my views of class, which he labels “economistic” and which he sees pervading the profession. One’s own sins are enough of a burden without having to carry the profession’s as well. But that isn’t my sin. I have never held the view of class Tyrrell imputes to me, and I agree that “Class, culture, and consciousness are not discrete categories. . . .” Unlike him, I do think there has to be some separation of terms for purposes of analysis; the English language and the human mind won’t permit anything else. Still, I think we are not far apart. Once more, we disagree on how well the book achieves goals we pretty much share.

I stand by my original judgment that *Sobering Up* has merit as a good work in a field needing good work. It would be too much to claim that it takes us very far toward that more sophisticated and precise social history Dr. Tyrrell and I both eventually hope to read and write.

RONALD G. WALTERS  
Johns Hopkins University

#### TO THE EDITOR:

Because of the shrill, distorted nature of Paul Conkin’s review of the third (post-1933) volume of my *Elites in American History* (AHR, 86 [1981]: 481), I would like to raise a question about the selection of the person chosen to review this work and to make some comments about the review itself.

Professor Conkin is primarily an intellectual historian and, judging from the record, has never done any substantial in-depth study of the socioeconomic background of governmental decision makers and its possible effect on their actions in office. Why was an individual from another field chosen to review the book? Surely there must be someone better qualified for the task.

In analyzing the socioeconomic background and actions of government officials, the basic standard I have employed is not one involving such moralistic concepts as “guilt” or “purity of heart,” as Professor Conkin put it, but the broad conflict-of-interest or public interest standard adopted by the Bar Association of the City of New York in its report on *Congress and the Public Trust* (a standard applicable to all government officials).

Further, the corporate directorships to which I referred are not “honorary directorships,” but the

formal (fully empowered) directorships held by persons at or around the time of their appointment to high federal office. I did not refer to the special category of honorary directors, which is given by some corporate enterprises to emeritus board members. Apparently Professor Conkin is not aware of the difference.

Finally, I would like to protest against the use of such terms as “corrupt,” “sinister,” and “corporate virus” in reference to corporate connections. I have never employed or thought in such terms and resent any inference that I did.

As for the relative merits of my work, the readers of American history will be the final judge.

PHILLIP H. BURCH, JR.  
Rutgers College

#### TO THE EDITOR:

Emiliana Noether’s review of my *Byzantium for Rome: The Politics of Nostalgia in Umbertian Italy, 1878–1900* (AHR, 86 [1981]: 614–15) requires a response from me lest the readers of the AHR retain an inadequate idea of the book’s purpose and structure.

The problem that I set for myself in *Byzantium for Rome* was to account for the strength and longevity of the aesthetic attitude in late nineteenth-century Italian intellectual politics, as other historians had done for the same attitude in France (César Graña), in Germany (Fritz Stern and George L. Mosse), and in Austria (William J. McGrath and Carl E. Schorske). My thesis is that in post-Risorgimento Italy aesthetic politics developed as a result of a provincial middle class in distress over “confiscatory taxes and a devastating downward spiral in prices and rents” (p. 218). The structure of the argument rests on an analysis of the work and careers of the principal spokesmen for the aesthetic attitude, Giosué Carducci and Gabriele D’Annunzio, in the specific historical context of their mutual association on Angelo Sommaruga’s journal, the *Cronaca bizantina* (1881–85) and in the aftermath of that publisher’s fall. Many other portraits are included in the book, notably that of Enrico Corradini, the founder of the Nationalist party, who began his career as a D’Annunzian aesthete. This is not to say, as Professor Noether did in her review, that *Byzantium for Rome* presents the *Cronaca bizantina*, D’Annunzio, and Corradini “in one neat package”; it is only to point out, with Marx, Mannheim, and Gramsci—the theorists who provided the major inspiration for my work—that history, to mean anything at all, must be the record of “particular events and particular actions, whose appearance at a particular moment can only be explained in concrete terms” (p. 220). The theoretical approach of the book seems to have escaped Noether’s notice, and I can find very little in



her review about my thesis or the structure of my argument.

Professor Noether dwelled on the manifold infelicities of my style. It would be tedious though by no means impossible to show how each of her stylistic criticisms is either trivial or erroneous, but *de gustibus non est disputandum*. No writer can expect to please everybody all of the time; however, the readers of the *AHR* should be informed that Noether's disesteem for my style is not the only word on this matter. I would like to refer them to the review of my book in the *Journal of Modern History*; there the reviewer praised the style as "fluent, supple, engagingly witty, well suited to communicate the detached, critical, yet deeply concerned point of view he maintains towards the politics of nostalgia in Umbertian Italy throughout his work."

I am also accused of hyperbole—this "perhaps unconsciously borrowed from D'Annunzio," Professor Noether gently suggested. She mentioned several examples of this defect, but for the sake of space I can only deal with one here, although it is typical of the aggravating carelessness with which she read my book. She claimed that I call Carducci the "Italian Aeschylus." Anyone who cares to read this passage (p. 9) will see how little attention Noether paid to my plain meaning, that Aeschylus was the sort of poet-soldier figure that Carducci, a classicist, "wanted to be"—a vastly different suggestion from the absurd one she has made me make in her review.

As for my translation of *superbamente* as superbly rather than as proudly, I refer Professor Noether to the 1961 Garzanti Italian-English dictionary where "superbly" is listed as one of the accepted meanings. The Latin root, *superbus*, can mean excellent, proud, haughty, or superb, and to quibble in print over one meaning or another strikes me as an act of invincible pedantry.

RICHARD DRAKE  
Princeton University

#### PROFESSOR NOETHER REPLIES:

As the author of a first book, Richard Drake justifiably rises to defend his work. I did not, however, write my review of it carelessly or in haste, as he seems to think. In fact, I read the book carefully twice, took many notes, and checked some sources to make sure that I would do Drake no injustice. After receiving a copy of his letter to the editor of the *AHR*, I reread the book for a third time and found that my criticism of it remains unchanged. Had I been less thorough in reviewing his book, I might have been less critical, for Dr. Drake has undoubtedly done a great deal of research on the subject matter, which can only impress a cursory reader.

Dr. Drake's theoretical approach did not escape me, and, like Drake, I believe that "history, to mean

anything at all, must be the record of particular events and particular actions, whose appearance at a particular moment can only be explained in concrete terms." My criticism rests on Drake's often injudicious choice of events and his juxtaposition of many of them, as well as on his omission of other pertinent facts, such as his failure to mention the articles written by D'Annunzio for *La Tribuna* of Rome from May 27 to July 6, 1888, which were republished in 1915 in *L'Armata d'Italia*. Nationalist in tone and highly critical of Parliament's failure to appropriate additional funds for the Italian navy, these articles support Drake's thesis of the links between disenchanted aesthetes and nationalism. As for Drake's style, *chacun à son goût*. I found it, and still find it, often irritating, distracting, and arch. Others are welcome to find it "fluent, supple, engagingly witty." What can I say?

Despite Dr. Drake's attempt to enlighten me on his rationale for calling Carducci the Italian Aeschylus, I must reiterate that I still consider this comparison, read together with some of his other descriptive fancies, somewhat hyperbolic. That Carducci might have wanted to be considered an Aeschylus-like figure may be true, but to label him the "Italian Aeschylus" does not accurately describe what Carducci represents in Italian literature.

With reference to *superbamente*, I thank Dr. Drake for his lecture on its Latin derivation. Unfortunately, I learned nothing new. To illustrate my point further on the difference between "proud" and "superb," let me note that, while Drake may be "proud" of his book, would he say that he is "superb" of it? Were he writing in Italian or Latin, *superbo* or *superbus* would be correct, but English has its own meanings. If he wishes to consider me invincibly pedantic, I accept the designation. I would rather be pedantic in my insistence on the need to render the meaning of words correctly when translating, than careless. The Italians knew well the dangers of translation: *traduttore, traditore*.

EMILIANA PASCA NOETHER  
University of Connecticut

#### TO THE EDITOR:

The review of *The Public Good: Philanthropy and Welfare in the War Era* (*AHR*, 86 [1981]: 653–54) states that a quotation from P. T. Barnum "comes at the end of the book, and is to no purpose other than display." Readers who consult page 216 of *The Public Good* will find that the quotation introduces a section on museum developments and is used to identify and characterize these developments. Choosing the quotation and finding the best place and most appropriate way to use it were among the many decisions made in writing the book.

My reason for bringing this minor point to the attention of your readers is that *The Public Good* is

not, as the reviewer seems to have assumed, a passive reflection of "what happened." What happens in the book is what the author has made happen as a result of his selection, organization, and presentation of material. As in any historical work, the author's aim is to mold the data into a convincing representation of reality. The responsibility assumed and the analytical energy expended in deciding what to include and what to leave out, which people and tendencies to emphasize and which to ignore, should not be underestimated.

Establishing what happened, as practicing historians know, is a demanding assignment. Failure to take the task seriously or to do it properly leads to errors in interpretation later students have to correct. What counts in history is not assertiveness or even conviction in pushing a point of view, but breadth of vision, accuracy in delineating events, sensitivity to fine distinctions, and fairness in appraising character and conduct.

ROBERT BREMNER  
*Ohio State University*

PROFESSOR HUGGINS REPLIES:

Robert Bremner's letter fails to reveal what he objects to in my review of *The Public Good*. It seems a

characteristic fault; the historian that Bremner limns need not have anything to say. That is why his book is disappointing. My review said no more than that. For all of its descriptive qualities, and despite a potentially exciting subject, it raises no important questions, and it answers none.

NATHAN I. HUGGINS  
*Harvard University*

TO THE EDITOR:

In his discerning and generally favorable review of my history of the Tennessee Valley Authority, *TVA: Bridge over Troubled Waters* (AHR, 86 [1981]: 666), Thomas K. McCraw stated that its "lengthy and complex story is informal to the point of chattiness." Since the book is for the general reader, this seemed the best approach. Furthermore, it is my studied opinion that what the writing and teaching of history need nowadays is more chattiness and less of esoteric treatment.

Coincidentally, the book was planned and written to a considerable extent under the supervision of my former teacher, the late Professor Ralph Hidy of Thomas McCraw's own Harvard Business School.

NORTH CALLAHAN  
*New York University*

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Compiled by JAMES F. GOODE

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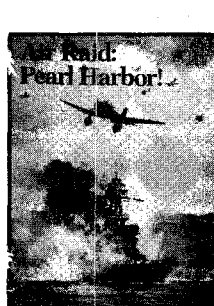
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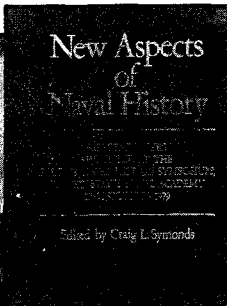


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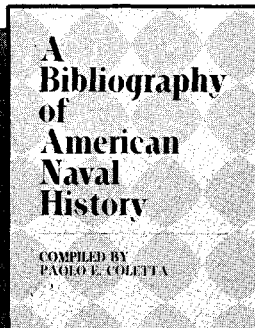


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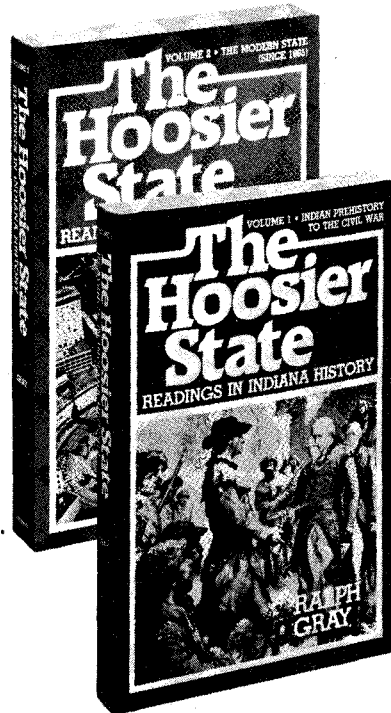
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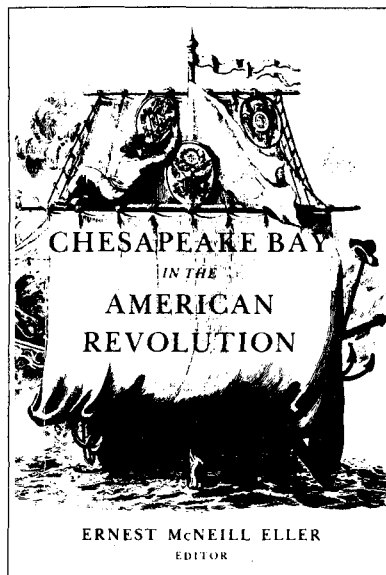
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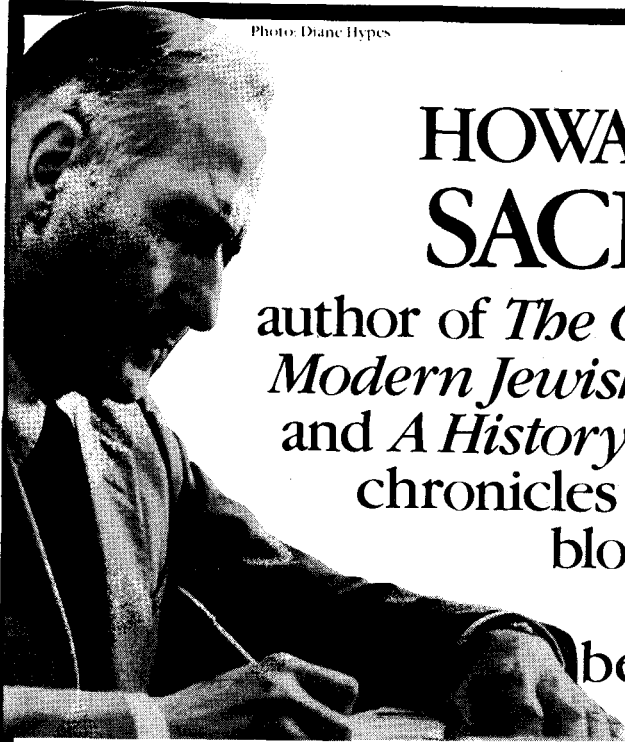
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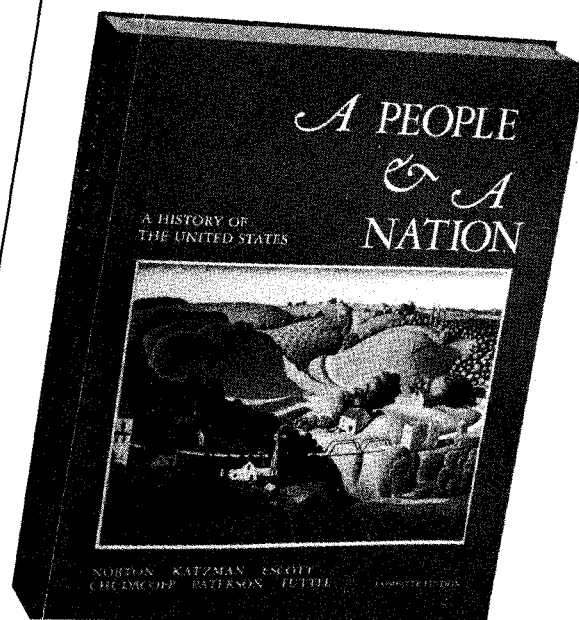
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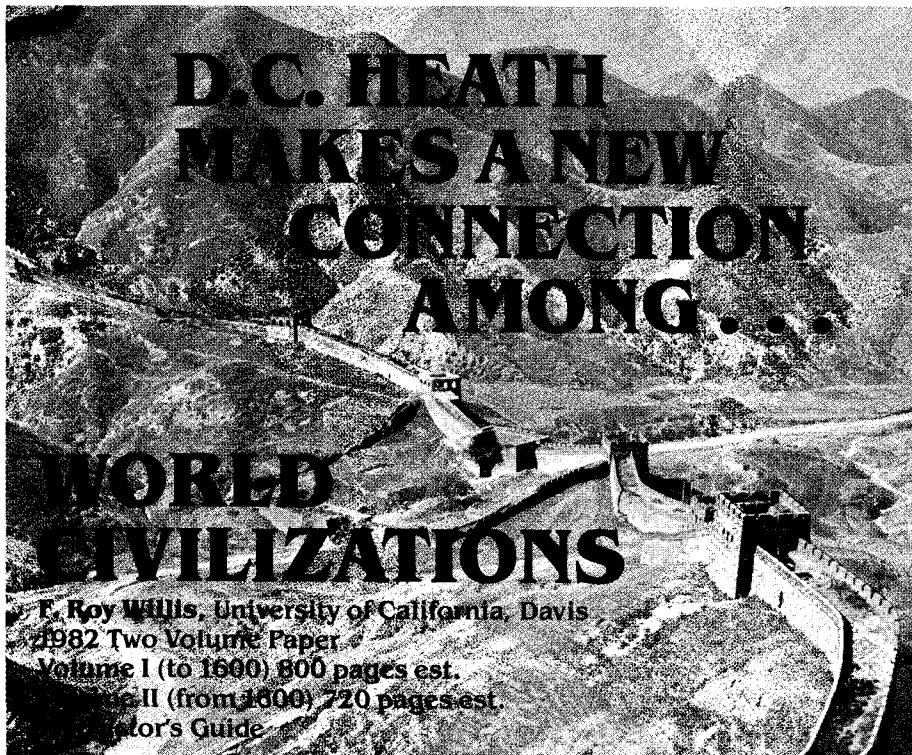
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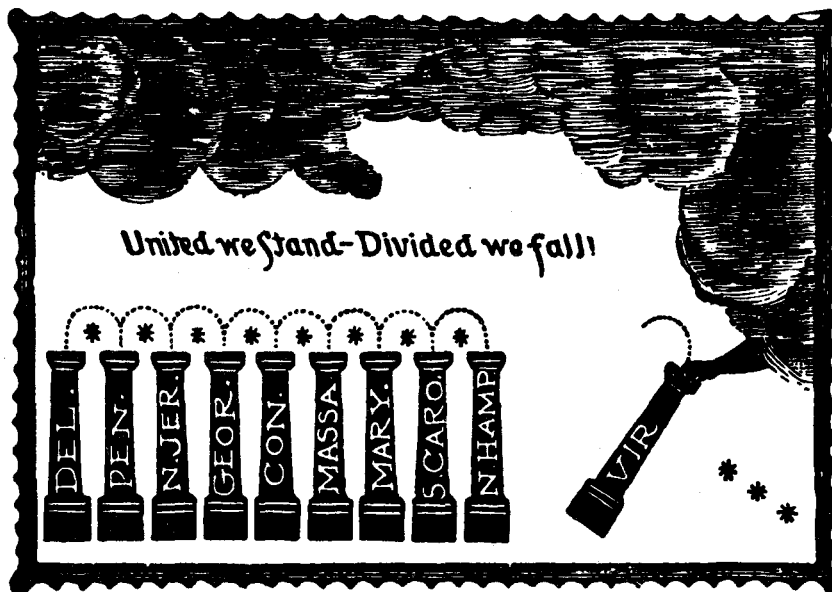
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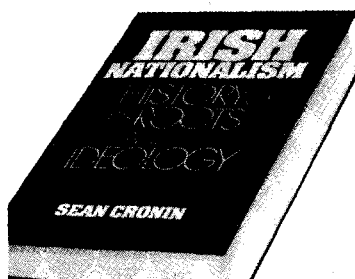
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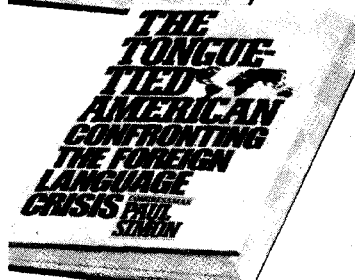
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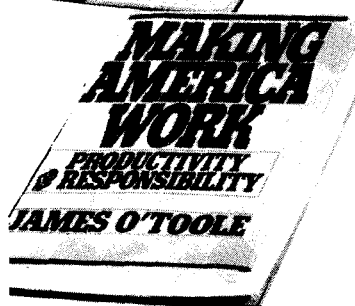
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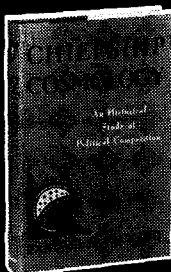
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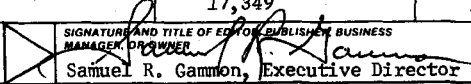
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